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ART. XI.—*Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies*;* by J. NORMAN LOCKYER, F.R.S.

IT is known to many Fellows of the Society that I have for the last four years been engaged upon the preparation of a map of the solar spectrum on a large scale, the work including a comparison of the Fraunhofer lines with those visible in the spectrum of the vapor of each of the metallic elements in the electric arc.

To give an idea of the thoroughness of the work, at all events in intention, I may state that the complete spectrum of the sun, on the scale of the working map, will be half a furlong (330 ft.) long; that to map the metallic lines and purify the spectra in the manner which has already been described to the Society, more than 100,000 observations have been made and about two thousand photographs taken.

In some of these photographs we have vapors compared with the sun, in others vapors compared with each other; and others again have been taken to show which lines are long and which are short in the spectra.

I may state in way of reminder that the process of purification consisted in this: When, for instance, an impurity of manganese was searched for in iron, if the longest line of Mn was absent, the short lines must also be absent on the hypothesis that the elements are elementary; if the longest line was present, then the impurity was traced down to the shortest line present.

* Paper read at the Royal Society, December 12, 1878.

TABLE I.—FINAL REDUCTION—IRON.

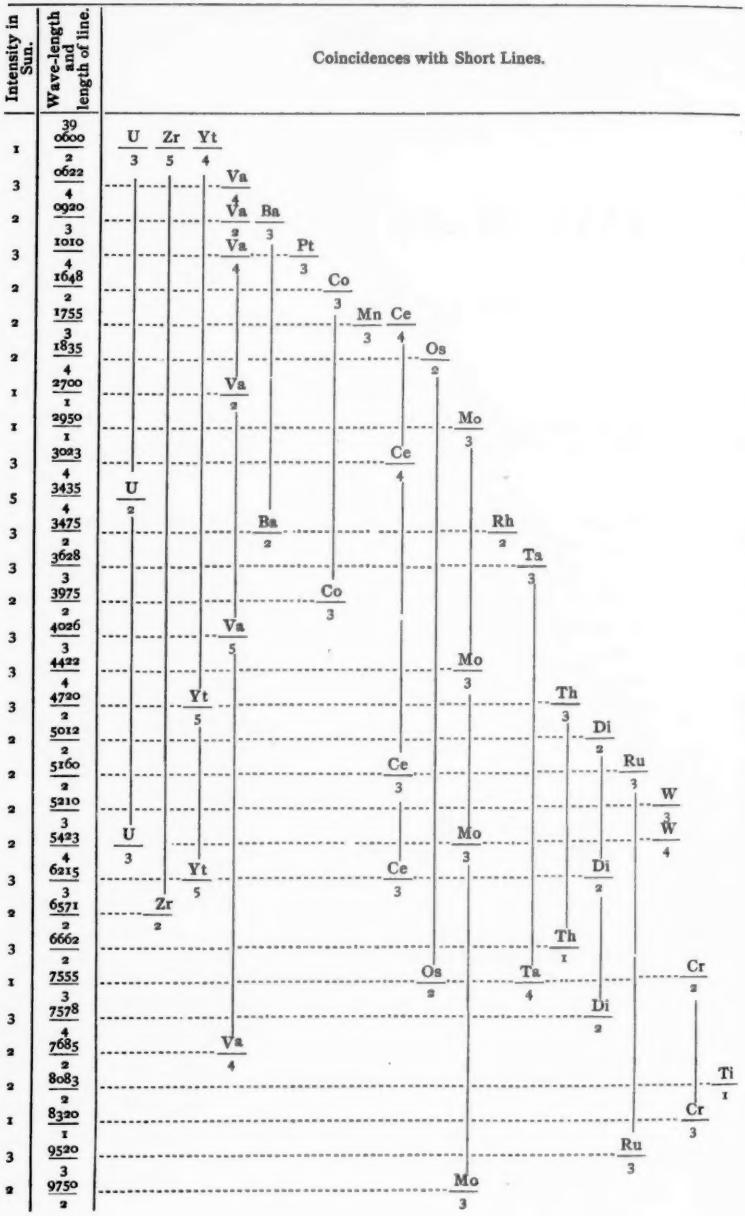
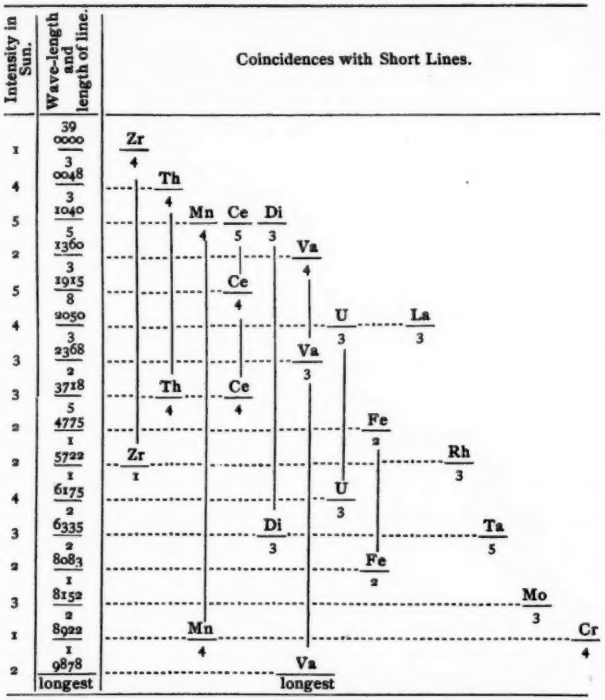


TABLE II.—FINAL REDUCTION—TITANIUM.



The Hypothesis that the Elements are Simple Bodies does not include all the Phenomena.

The final reduction of the photographs of all the metallic elements in the region 39–40, a reduction I began in the early part of the present year, and which has taken six months, summarized all the observations of metallic spectra, compared with the Fraunhofer lines, accumulated during the whole period of observation. Now this reduction has shown me that the hypothesis that identical lines in different spectra are due to impurities is not sufficient. I shall show in detail in a subsequent paper the hopeless confusion in which I have been landed. I limit myself on the present occasion to giving tables showing how the hypothesis deals with the spectra of iron and titanium.

We find short line coincidences between many metals the impurities of which have been eliminated or in which the freedom from mutual impurity has been demonstrated by the absence of the longest lines.

Evidences of Celestial Dissociation.

It is five years since I first pointed out that there are many facts and many trains of thought suggested by solar and stellar physics which point to another hypothesis—namely, *that the elements themselves, or at all events some of them, are compound bodies.*

In a letter written to M. Dumas, December 3, 1873, and printed in the Comptes Rendus, I thus summarized a memoir which has since appeared in the Philosophical Transactions.

"Il semble que plus une étoile est chaude plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques.*

"Ainsi nous avons:

"1. Des étoiles très-brillantes où nous ne voyons que l'hydrogène, *en quantité énorme*, et le magnésium;

"2. Des é toiles plus froides, comme notre Soleil, où nous trouvons:

$$H+Mg+Na$$
$$H+Mg+Na+Ca,\ Fe,\ \ldots;$$

dans ces étoiles, pas de métalloïdes;

"3. Des étoiles plus froides encore, dans lesquelles *tous les éléments métalliques sont* ASSOCIÉS, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des métalloïdes et des composés.

"4. Plus une étoile est âgée, *plus l'hydrogène libre disparaît;* sur la terre, nous ne trouvons plus d'hydrogène en liberté.

"Il me semble que ces faits sont les preuves de plusieurs idées émises par vous. J'ai pensé que nous pouvions imaginer une '*dissociation céleste*,' qui continue le travail de nos fourneaux, et que les métalloïdes sont des composés qui sont dissociés par la température solaire, pendant que les éléments métalliques monatomiques, dont les poids atomiques sont les moindres, son précisément ceux qui résistent, même à la température des étoiles les plus chaudes."

Before I proceed further, I should state that while observations of the sun have since shown that calcium should be introduced between hydrogen and magnesium for that luminary, Dr. Huggins' photographs have demonstrated the same fact for the stars, so that in the present state of our knowledge, independent of all hypotheses, the facts may be represented as follows:—

Hottest Stars	Lines of	H+Ca+Mg
Sun	Lines of	H+Ca+Mg+Na+Fe
Cooler Stars	Lines of	— — Mg+Na+Fe+Bi+Hg
Coolest	Fluted bands of	— — — — — — Metalloids.

* This referred to the old numbers in which Mg=12, Na=23.

Following out these views, I some time since communicated a paper to the Society on the spectrum of calcium, to which I shall refer more expressly in the sequel.

Differentiation of the Phenomena to be observed on the two Hypotheses.

When the reductions of the observations made on metallic spectra, on the hypothesis that the elements were really elementary, had landed me in the state of utter confusion to which I have already referred, I at once made up my mind to try the other hypothesis, and therefore at once sought for a critical differentiation of the phenomena on the two hypotheses.

Obviously the first thing to be done was to inquire whether one hypothesis would explain these short line coincidences which remained after the reduction of all the observations on the other. Calling for sake of simplicity the short lines common to many spectra *basic lines*, the new hypothesis, to be of any value, should present us with a state of things in which basic molecules representing bases of the so-called elements should give us their lines, varying in intensity from one condition to another, the *conditions* representing various compoundings.

Suppose A (nickel, let us say) to contain B (cobalt) as an impurity and as an element, what will be the difference in the spectroscopic result?

A in both cases will have a spectrum of its own;

B as an impurity will add its lines according to the amount of impurity, as I have shown in previous papers.

B as an element will add its lines according to the amount of dissociation, as I have also shown.

The difference in the phenomena, therefore, will be that, with gradually increasing temperature, the spectrum of A *will fade*, if it be a compound body, as it will be increasingly dissociated, and it *will not* fade if it be a simple one.

Again, on the hypothesis that A is a compound body, that is, one compounded of at least two similar or dissimilar molecular groupings, then the longest lines at one temperature will not be the longest at another, the whole fabric of "impurity elimination," based upon the assumed single molecular grouping, falls to pieces, and the origin of the basic lines is at once evident.

This may be rendered clearer by some general considerations of another order.

General Considerations.

Let us assume a series of furnaces A . . . D, of which A is the hottest.

Let us further assume that in A there exists a substance *a*

by itself competent to form a compound body β by union with itself or with something else when the temperature is lowered.

Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be the only one visible in B, as the spectrum of the assumed elementary body a would be the only one visible in A.

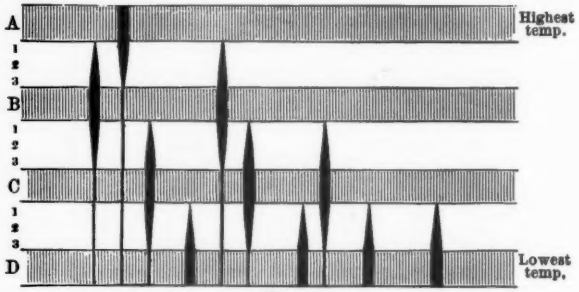


Fig. 1.

A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

Now if into the furnace A we throw some of this doubly compound body γ we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β, and finally a would exist alone, and the spectrum would be reduced to one of the utmost simplicity.

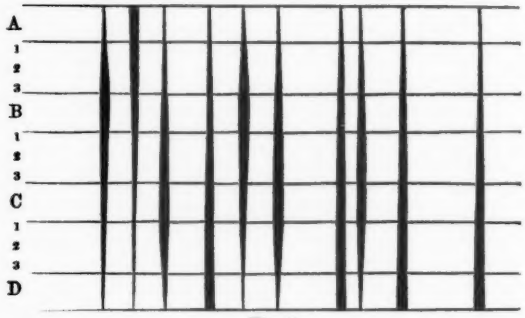


Fig. 2.

This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ and δ, all higher, that is, more compound forms of a, and although the strong lines in the diagram may represent the true spectra of

these substances in the furnaces B, C and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines.* Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

In short, the line of *a*, strong in A, is *basic* in B, C and D, the lines of β, strong in B, are *basic* in C and D, and so on.

I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ and δ into *a* is far below that stage, although higher than B.

It will be seen from this diagram that then the only difference in the spectra of the bodies existing in the four furnaces would consist merely in the relative thicknesses of the lines. The spectra of the substances as they exist in A would contain as many lines as would the spectra of the substances as they exist in D; each line would in turn be basic in the whole series of furnaces instead of in one or two only.

Applications of these General Considerations to Impurity Elimination.

Now let us suppose that in the last diagram (fig. 2) the four furnaces represent the spectra of say, iron, broken up into different finenesses by successive stages of heat. It is first of all abundantly clear that the relative thicknesses of the iron lines observed will vary according as the temperature resembles that of A, B, C, or D. The positions in the spectra will be the same, but the intensities will vary; this is the point. The longest lines, represented in the diagram by the thickest ones, will vary as we pass from one temperature to another. It is on this ground that I have before stated that the whole fabric of impurity elimination must fall to pieces on such an hypothesis. Let us suppose, for instance, that manganese is a compound of the form of iron represented in furnace B, with something else; and suppose again that the photograph of iron which I compare with manganese represents the spectrum of the vapor at the temperature of the furnace D. To eliminate the impurity of iron in manganese, as I have eliminated it, we begin the search by looking for the longest and strongest lines shown in the photograph of iron, in the photograph of manganese taken under the same conditions. I do not find these lines. I say, therefore, that there is no impurity of iron in manganese, but although the longest iron lines are not there, some of the faintest basic ones are. This I hold to be the explanation of the apparent confusion in which we are landed on the supposition that the elements are elementary.

Application of these Considerations to Known Compounds.

Now to apply this reasoning to the dissociation of a known compound body into its elements.—

A compound body, such as a salt of calcium, has as definite a spectrum as a simple one; but while the spectrum of the metal itself consists of lines, the number and thickness of some of which increase with increased quantity, the spectrum of the compound consists in the main of channelled spaces and bands, which increase in like manner.

In short, the molecules of a simple body and a compound one are affected in the same manner by quantity in so far as their spectra are concerned; *in other words, both spectra have their long and short lines*, the lines in the spectrum of the element being represented by bands or fluted lines in the spectrum of the compound; and in each case the greatest simplicity of the spectrum depends upon the smallest quantity, and the greatest complexity (a continuous spectrum) upon the greatest.

The heat required to act upon such a compound as a salt of calcium so as to render its spectrum visible, dissociates the compound according to its volatility; the number of true metallic lines which thus appear is a measure of the quantity of the metal resulting from the dissociation, and as the metal lines increase in number, the compound bands thin out.

I have shown in previous papers how we have been led to the conclusion that binary compounds have spectra of their own, and how this idea has been established by considerations having for a basis the observations of the long and short lines.

It is absolutely similar observations and similar reasoning which I have to bring forward in discussing the compound nature of the chemical elements themselves.

In a paper communicated to the Royal Society in 1874, referring, among other matters, to the reversal of some lines in the solar spectrum, I remarked,*—

"It is obvious that greater attention will have to be given to the precise *character* as well as to the position of each of the Fraunhofer lines, in the thickness of which I have already observed several anomalies. I may refer more particularly at present to the two H lines 3933 and 3968 belonging to calcium, which are much thicker in all photographs of the solar spectrum [I might have added that they were by far the thickest lines in the solar spectrum] than the largest calcium line of this region (4226·3), this latter being invariably thicker than the H lines in all photographs of the calcium spectrum, and remaining, moreover, visible in the spectrum of substances containing calcium in such small quantities as not to show any traces of the H lines.

* Phil. Trans., vol. clxiv, part 2, p. 807.

"How far this and similar variations between photographic records and the solar spectrum are due to causes incident to the photographic record itself, or to variations in the intensities of the various molecular vibrations under solar and terrestial conditions, are questions which up to the present time I have been unable to discuss."

An Objection Discussed.

I was careful at the very commencement of this paper to point out that the conclusions I have advanced are based upon the analogies furnished by those bodies which, by common consent and beyond cavil and discussion are compound bodies. Indeed, had I not been careful to urge this point the remark might have been made that the various changes in the spectra to which I shall draw attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line, due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular grouping of hydrogen as that which gives us two green lines only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green lines of hydrogen without the red one.

I submit, therefore, that the argument founded on the overnotes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures is, as I shall show, as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other.

Inquiry into the Probable Arrangement of the Basic Molecules.

As the results obtained from the above considerations seemed to be so far satisfactory, inasmuch as they at once furnished an explanation of the *basic lines* actually observed, the inquiry was thought worthy of being carried to a further stage.

The next point I considered was to obtain a clear mental view of the manner in which, on the principle of evolution, various bases might now be formed, and then become basic themselves.

It did not seem unnatural that the bases should increase their complexity by a process of continual multiplication, the factor being 1, 2, or even 3, if conditions were available under which the temperature of their environment should decrease, as we imagined it to do from the furnace A down to furnace D. This would bring about a condition of molecular complexity in which the proportion of the molecular weight of a substance so produced in a combination with another substance would go on continually increasing.

Another method of increasing molecular complexity would be represented by the addition of molecules of different origins. Representing the first method by A+A, we could represent the second by A+B. A variation of the last process would consist in a still further complexity being brought about by the addition of another molecule of B, so that instead of $(A+B)_2$ merely, we should have $A+B_2$.

Of these three processes the first one seemed that which it was possible to attack under the best conditions, because the consideration of impurities was eliminated; the prior work has left no doubt upon the mind about such and such lines being due to calcium, others to iron, and so forth. The inquiry took this form, granting that these lines are special to such and such a substance, does each become basic in turn as the temperature is changed?

I therefore began the inquiry by reviewing the evidence concerning calcium and seeing if hydrogen, iron and lithium behaved in the same way.

Application of the above Views to Iron, Lithium, and Hydrogen.

Calcium.—It was in a communication to the Royal Society made some time ago (Proceedings, vol. xxii, p. 380, 1874), that I first referred to the possibility that the well-known line-spectra of the elementary bodies might not result from the vibration of similar molecules. I was led to make the remark in consequence of the differences to which I have already drawn attention in the spectra of certain elements as observed in the spectrum of the sun and in those obtained with the ordinary instrumental appliances.

Later (Proc. Roy. Soc., No. 168, 1876) I produced evidence that the molecular grouping of calcium which, with a small induction-coil and small jar, gives a spectrum with its chief line in the blue, is nearly broken up in the sun, and quite broken up in the discharge from a large coil and jar, into another or others with lines in the violet.

I said "another," or "others," because I was not then able to determine whether the last named lines proceeded from the same or different molecules; and I added that it was possible we might have to wait for photographs of the spectra of the brighter stars before this point could be determined.

I also remarked that this result enabled us to fix with very considerable accuracy the electric dissociating conditions which are equivalent to that degree of dissociation at present at work in the sun.

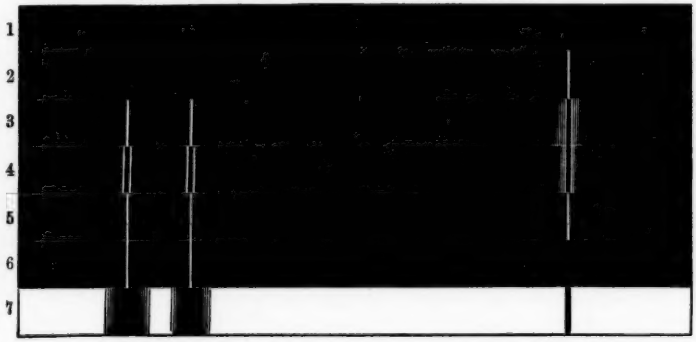


FIG. 3.—The blue end of the spectrum of calcium under different conditions. 1. Calcium is combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen. 2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current. 3. The spectrum of metallic calcium in the electric arc with a small number of cells. 4. The same when the number of cells is increased. 5. The spectrum when a coil and small jar are employed. 6. The spectrum when a large coil and large jar are used. 7. The absorption of the calcium vapor in the Sun.

In fig. 3 I have collected several spectra copied from photographs in order that the line of argument may be grasped.

First we see what happens to the non-dissociated and the dissociated chloride. Next we have the lines with a weak voltaic arc, the single line to the right (W. L. 4226·3) is much thicker than the two lines (W. L. 3933 and 3968) to the left, and reverses itself.

We have next calcium exposed to a current of higher tension. It will be seen that here the three lines are almost equally thick, and all reverse themselves.

Now it will be recollected, that in the case of known compounds the band structure of the true compounds is reduced as dissociation works its way, and the spectrum of each constituent element makes its appearance. If in 3 we take the wide line as representing the banded spectrum of the compound, and the thinner ones as representing the longest elemental lines

making their appearance as the result of partial dissociation, we have, by hypothesis, an element behaving like a compound.

If the hypothesis be true, we ought to be able not only to obtain, with lower temperatures, a still greater preponderance of the single line, *as we do;* but with higher temperatures, a still greater preponderance of the double ones, *as we do.*

I tested this in the following manner: Employing photography, because the visibility of the more refrangible lines is small, and because a permanent record of an experiment, free as it must be from all bias, is a very precious thing.

Induced currents of electricity were employed in order that all the photographic results might be comparable.

To represent the lowest temperature I used a small induction coil and a Leyden jar only just large enough to secure the requsite amount of photographic effect. To represent the highest, I used the largest coil and jar at my disposal. The spark was then taken between two aluminium electrodes, the lower one cup-shaped, and charged with a salt of calcium.

In the figure I give exact copies of the results obtained. It will be seen that with the lowest temperature only the single line (2) and with the highest temperature only the two more refrangible lines (6) are recorded on the plate.

This proved that the intensity of the vibrations was quite changed in the two experiments.

Perhaps it may not be superfluous here to state the reasons which induced me to search for further evidence in the stars.

It is abundantly clear that if the so-called elements, or more properly speaking their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end.

That is the first point.

The second is this:—

We are justified in supposing that our "calcium," once formed, is a distinct entity, whether it be an element or not, and therefore, by working at it alone, we should never know whether the temperature produces a single simpler form or more atomic condition of the same thing, or whether we actually break it up into X+Y, because neither X nor Y will ever vary

But if calcium be a product of a condition of relatively lower temperature, then in the stars, hot enough to enable its constituents to exist uncompounded, we may expect these constituents to vary in quantity; there may be more of X in one star and more of Y in another; and if this be so, then the H and K lines will vary in thickness, and the extremest limit of variation will be that we shall only have H representing, say X in one star, and only have K representing, say Y in another. Intermediately between these extreme conditions we may have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others.

Professor Stokes was good enough to add largely to the value of my paper as it appeared in the *Proceedings*, by appending a note pointing out that "When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature. It would be in accordance with analogy to suppose that as a rule the same would take place in an incandescent surface, though in this case the spectrum would be discontinuous instead of continuous. Thus if A, B, C, D, E denote conspicuous bright lines of increasing refrangibility, in the spectrum of the vapor, it might very well be at that a comparatively low temperature A should be the brightest and the most persistent; at a higher temperature, while all were brighter than before, the relative brightness might be changed, and C might be the brightest and the most persistent, and at a still higher temperature E."

On these grounds Professor Stokes, while he regarded the facts I mentioned as evidence of the high temperature of the sun, did not look upon them as *conclusive* evidence of the dissociation of the molecule of calcium.

Since that paper was sent in, however, the appeal to the stars to which I referred in it has been made, and made with the most admirable results, by Dr. Huggins.

The result of that appeal is that the line which, according to Professor Stokes' view, should have prevailed over all others, as Sirius is acknowledged to be a hotter star than our sun, is that, if it exists at all in the spectrum, it is so faint that it was not recognized by Dr. Huggins in the first instance.

In Sirius, indeed, the H line due to one molecular grouping of calcium is as thick as are the hydrogen lines as mapped by Secchi, while the K line, due to another molecular grouping, which is equally thick in the spectrum of the sun, has not yet made its appearance.

In the sun, where it is as thick as H, the hydrogen lines have vastly thinned.

While this paper has been in preparation, Dr. Huggins has been good enough to communicate to me the results of his most important observations, and I have also had an opportunity of inspecting several of the photographs which he has recently taken. The result of the recent work has been to show that H and *h* are of about the same breadth in Sirius. In *α* Aquilæ while the relation of H to *h* is not greatly changed, a distinct approach to the solar condition is observed, K being now unmistakably present, although its breadth is small as compared with that of H. I must express my obligations to Dr. Huggins for granting me permission to enrich my paper by reference to these unpublished observations. His letter, which I have permission to quote, is as follows:—

"It may be gratifying to you to learn that in a photograph I have recently taken of the spectrum of *α* Aquilæ there is a line corresponding to the more refrangible of the solar H lines [that is K], but about half the breadth of the line corresponding to the first H lines.

In the spectra of *α* Lyræ and Sirius the second line is absent."

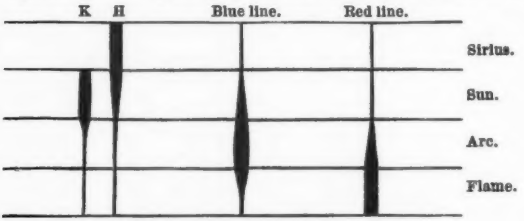


Fig. 4.—The Molecular Groupings of Calcium.

Professor Young's observations of the chromospheric line, to which I shall afterward refer, give important evidence regarding the presence of calcium in the chromosphere of the sun. He finds that the H and K lines of calcium are strongly reversed in every important spot, and that in solar storms H has been observed injected into the chromosphere seventy-five times, and K fifty times, while the blue line at W. L. 4226·3, the all-important line at the arc-temperature, was only injected thrice.

Further, in the eclipse observed in Siam in 1875, the H and K lines left the strongest record in the spectrum of the chromosphere, while the line near G in a photographic region of much greater intensity was not recorded at all. In the American eclipse of the present year the H and K lines of calcium were distinctly visible at the base of the corona, in which for the first time the observer could scarcely trace the existence of any hydrogen.

To sum up, then, the facts regarding calcium, we have first of all the H-line differentiated from the others by its almost solitary existence in Sirius. We have the K-line differentiated from the rest by its birth, so to speak, in *α* Aquilæ, and the thickness of its line in the sun, as compared to that in the arc. We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence in solar storms in which they are almost universally seen, and finally, by its absence during eclipses, while the H and K lines have been the brightest seen or photographed. Last stage of all, we have calcium, distinguished from its salts by the fact that the blue line is only visible when a high temperature is employed, each salt having a definite spectrum of its own, in which none of the lines to which I have drawn attention appear, so long as the temperature is kept below a certain point.

Iron.

With regard to the iron spectrum I shall limit my remarks to that portion of it visible on my photographic plates between H and G. It may be described as a very complicated spectrum so far as the number of lines is concerned in comparison with such bodies as sodium and potassium, lead, thallium, and the like, but unlike them again it contains no one line which is clearly and unmistakably reversed on all occasions. Compared, however, with the spectrum of such bodies as cerium and uranium the spectrum is simplicity itself.

Now among these lines are two triplets, two sets of three lines each, giving us beautiful examples of those repetitions of structure in the spectrum which we meet with in the spectra of almost all bodies, some of which have already been pointed out by Mascart, Cornu, and myself. Now the facts indicate that these two triplets are not due to the vibration of the same molecular grouping which gives rise to most of the other lines. They are as follows: In many photographs in which iron has been compared with other bodies, and in others again in which iron has been photographed as existing in different degrees of impurity in other bodies, these triplets have been seen almost alone, and the relative intensity of them, as compared with the few remaining lines, is greatly changed. In this these photographs resemble one I took three years ago, in which a large coil and jar were employed instead of the arc, which necessitated an exposure of an hour instead of two minutes. In this the triplet near G is very marked, the two adjacent lines more refrangible near it, which are seen nearly as strong as the triplet itself, in some of the arc photographs I possess, are only

very faintly visible, while dimmer still are seen the lines of the triplet between H and *h*.

There is another series of facts in another line of work. In solar storms, as is well known, the iron lines sometimes make their appearance in the chromosphere. Now, if we were dealing here with one molecular grouping, we should expect the lines to make their appearance in the order of their lengths, and we should expect the shortest lines to occur less frequently than the longest ones. Now, precisely the opposite is the fact. One of the most valuable contributions to solar physics that we possess is the memoir in which Professor C. A. Young records his observation of the chromospheric lines, made on behalf of the United States Government, at Shumar, in the Rocky Mountains. The glorious climate and pure air of this region, to which I can personally testify, enabled him to record phenomena which it is hopeless to expect to see under less favorable conditions. Among these were injections of iron vapor into the chromosphere, the record taking the form of the number of times any one line was seen during the whole period of observation.

Now two very faint and short lines close to the triplet near G were observed to be injected thirty times, while one of the lines of the triplet was only injected twice.

The question next arises, are the triplets produced by one molecular grouping or by two? This question I also think the facts help us to answer. I will first state by way of reminder that in the spark photograph the more refrangible triplet is barely visible, while the one near G is very strong. Now if one molecular grouping alone were in question this relative intensity would always be preserved however much the absolute intensity of the compound system might vary, but if it is a question of two molecules we might expect that in some of the regions open to our observation we should get evidence of cases in which the relative intensity is reserved or the two intensities are assimilated. What might happen does happen: the relative intensity of the two triplets in the spark photograph is grandly reversed in the spectrum of the sun. The lines barely visible in the spark photograph are among the most prominent in the solar spectrum, while the triplet which is strong in that photograph is represented by Fraunhofer lines not half so thick. Indeed, while the hypothesis that the iron lines in the region I have indicated are produced by the vibration of one molecule does not include all the facts, the hypothesis that the vibrations are produced by at least three distinct molecules includes all the phenomena in a most satisfactory manner.

Lithium.

Before the maps of the long and short lines of some of the chemical elements compared with the solar spectra, which were published in the Phil. Trans. for 1873, "Plate IX," were communicated to the Society, I very carefully tested the work of prior observers on the non-coincidence of the red and orange lines of that metal with the Fraunhofer lines, and found that neither of them were strongly if at all represented in the sun, and this remark also applies to a line in the blue at wave-length 4603.

The photographic lithium line, however, in the violet, has a strong representative among the Fraunhofer lines.

Applying, therefore, the previous method of stating the facts, the presence of this line in the sun differentiates it from all the others. For the differentiation of the red and yellow lines I need only refer to Bunsen's spectral analytical researches, which were translated in the Philosophical Magazine, December, 1875.

In Plate IV, two spectra of the lithium chloride are given, one of them showing the red line strong and the yellow one feeble, the other showing merely a trace of the red line, while the intensity of the yellow one is much increased, and a line in the blue is indicated. Another notice of the blue line of lithium occurs in a discourse by Professor Tyndall, reprinted in the Chemical News, and a letter of Dr. Frankland's to Professor Tyndall, dated November 7, 1861. This letter is so important for my argument, that I reprint it entire from the Philosophical Magazine, vol. xxii, p. 472:—

"On throwing the spectrum of lithium on the screen yesterday, I was surprised to see a magnificent blue band. At first I thought the lithium chloride must be adulterated with strontium, but on testing it with Steinheil's apparatus it yielded normal results without any trace of a blue band. I am just now reading the report of your discourse in the Chemical News, and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the cone points or ignited air guilty of its production? I find there blue bands with common salt, but they have neither the definiteness nor the brilliancy of the lithium band. When lithium wire burns in air it emits a somewhat crimson light; plunge it into oxygen, and the light changes to bluish white. This seems to indicate that a high temperature is necessary to bring out the blue ray."

POSTSCRIPT, Nov. 22, 1861.—I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line depends entirely on the temperature. The spectrum of lithium chloride, ignited in a Bunsen's burner flame, does not disclose the faintest trace of

the blue line; replace the Bunsen's burner by a jet of hydrogen (the temperature of which is higher than that of the Bunsen's burner) and the blue line appears, faint, it is true, but sharp and quite unmistakable. If oxygen now be solely turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum, and thus put an end to the experiment."

These observations of Professors Tyndall and Frankland differentiate this blue line from those which are observed at low temperatures. The line in the violet to which I have already referred, is again differentiated from all the rest by the fact that it is the only line in the spectrum of the sun which is strongly reversed, so far as our present knowledge extends. The various forms of lithium, therefore, may be shown in the following manner.

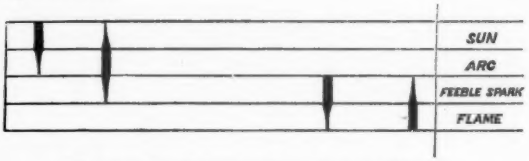


Fig. 5.—The Molecular Groupings of Lithium.

It is remarkable that in the case of this body which at relatively low temperature goes through its changes, its compounds are broken up at the temperature of the Bunsen burner. The spectrum, e. g. of the chloride, so far as I know, has never been seen.

Hydrogen.

All the phenomena of variability and inversion in the order of intensity presented to us in the case of calcium can be paralleled by reference to the knowledge already acquired regarding the spectrum of hydrogen.

Dr. Frankland and myself were working together on the subject in 1869. In that year (Proc., No. 112) we pointed out that the behavior of the *h* line was *hors ligne*, and that the whole spectrum could be reduced to one line, F.

"1. The Fraunhofer line on the solar spectrum, named *h* by Ångström, which is due to the absorption of hydrogen, is not visible in the tubes we employ with low battery and Leyden-jar power; it may be looked upon, therefore, as an indication of relatively high temperature. As the line in question has been reversed by one of us in the spectrum of the chromosphere, it follows that the chromosphere, when cool enough to absorb, is still of a relatively high temperature.

"2. Under certain conditions of temperature and pressure,

the very complicated spectrum of hydrogen is reduced in our instrument *to one line in the green* corresponding to F in the solar spectrum."

As in the case of calcium also, solar observation affords us most precious knowledge. The *h* line was missing from the protuberances in 1875, as will be shown from the accompanying extract from the Report of the Eclipse Expedition of that year:—

"During the first part of the eclipse two strong protuberances close together are noticed; on the limb towards the end these are partially covered, while a series of protuberances came out at the other edge. The strongest of these protuberances are repeated three times, an effect of course of the prism, and we shall have to decide if possible the wave-lengths corresponding to the images. We expect *a priori* to find the hydrogen lines represented. We know three photographic hydrogen lines: F, a line near G, and *h*. F is just at the limit of the photographic part of the spectrum, and we find indeed images of protuberances towards the less refrangible part at the limit of photographic effect. For, as we shall show, a continuous spectrum in the lower parts of the corona has been recorded, and the extent of this continuous spectrum gives us an idea of the part of the spectrum in which each protuberance line is placed. We are justified in assuming, therefore, as a preliminary hypothesis, that the least refrangible line in the protuberance shown on the photograph is due to F, and we shall find support of this view in the other lines. In order to determine the position of the next line the dispersive power of the prism was investigated. The prism was placed on a goniometer table in minimum deviation for F, and the angular distance between F and the hydrogen line near G, i. e. Hγ, was found, as a mean of several measurements to be 3′. The goniometer was graduated to 15″, and owing to the small dispersive power, and therefore relatively great breadth of the slit, the measurement can only be regarded as a first approximation. Turning now again to our photographs, and calculating the angular distance between the first and second ring of protuberances, we find that distance to be 3′ 15″. We conclude, therefore, that this second ring is due to hydrogen. We, therefore, naturally looked for the third photographic hydrogen line, which is generally called *h*, but we found no protuberance on our photographs corresponding to that wave-length. Although this line is always weaker than Hγ, its absence on the photograph is rather surprising, if it be not due to the fact that the line is one which only comes out at a high temperature. This is rendered likely by the researches of Frankland and Lockyer (Proc. Roy. Soc., vol. xvii, p. 453).

"We now turn to the last and strongest series of protuberances shown on our photographs. The distance between this series and the one we have found reason for identifying with $H\gamma$ is very little greater than that between $H\beta$ and $H\gamma$. Assuming the distances equal, we conclude that the squares of the inverse wave-lengths of the three series are in arithmetical progression. This is true as a first approximation. We then calculated the wave-length of this unknown line, and found it to be approximately somewhat smaller than 3,957 tenth-meters. No great reliance can be placed, of course, on the number, but it appears that the line must be close to the end of the visible spectrum.

"In order to decide if possible what this line is due to, we endeavored to find out both by photography and fluorescence whether hydrogen possesses a line in that part of the spectrum. We have not at present come to any definite conclusion. In vacuum tubes prepared by Geissler containing hydrogen, a strong line more refrangible than H is seen, but these same tubes show between $H\gamma$ and $H\delta$, other lines known not to belong to hydrogen, and the origin of the ultra-violet line is therefore difficult to make out. We have taken the spark in hydrogen at atmospheric pressures, as impurities are easier to eliminate, but a continuous spectrum extends over the violet and part of the ultra-violet, and prevents any observation as to lines. We are going on with experiments to settle this point.

"Should it turn out that the line is not due to hydrogen, the question will arise what substance it is due to. It is a remarkable fact that the calculated wave-length comes very close to H. Young has found that these calcium lines are always reversed in the penumbra and immediate neighborhood of every important sun-spot, and calcium must therefore go up high into the chromosphere. We draw attention to this coincidence, but our photographs do not allow us to draw any certain conclusions.

"At any rate, it seems made out by our photographs that the photographic light of the protuberances is in great part due to an ultra-violet line which does not certainly belong to hydrogen. The protuberances as photographed by this ultra-violet ray seem to go up higher than the hydrogen protuberances, but this may be due to the relative greater length of the line."

In my remarks upon calcium I have already referred to the fact that the line which our observation led us to believe was due to calcium in 1875, was traced to that element in this year's eclipse. The observations also show the curious connection that, at the time when the hydrogen lines were most brilliant in the corona, the calcium lines were not detected; next, when the

hydrogen lines, being still brilliant, the *h* line was not present (a condition of things which, in all probability, indicated a reduction of temperature), calcium began to make itself unmistakably visible; and finally, when the hydrogen lines are absent, they become striking objects in the spectrum of the corona.

To come back to *h*, then, I have shown that Dr. Frankland and myself, in 1869, found that it only made its appearance when a high tension was employed. We have seen that it was absent from among the hydrogen lines during the eclipse of 1875.

I have now to strengthen this evidence by the remark that it is always the shortest line of hydrogen in the chromosphere.

I now pass to another line of evidence.

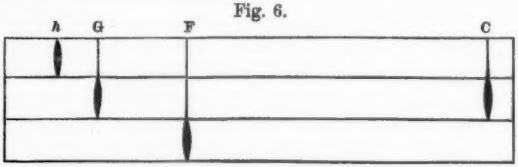
I submit to the Society a photograph of the spectrum of indium, in which, as already recorded by Thalèn, the strongest line is one of the lines of hydrogen (*h*), the other line of hydrogen (near G) being absent. I have observed the C line in the spark produced by the passage of an induced current between indium poles in dry air.

As I am aware how almost impossible it is to render air perfectly dry, I made the following differential experiment. A glass tube with two platinum poles about half an inch apart was employed. Through this tube a slow current of air was driven after passing through a U tube one foot high, containing calcium chloride, and then through sulphuric acid in a Wolff's bottle. The spectrum of the spark passing between the platinum electrodes was then observed, a coil with five Grove cells and a medium sized jar being employed. Careful notes were made of the brilliancy and thickness of the hydrogen lines as compared with those of air. This done, a piece of metallic indium which was placed loose in the tube, was shaken so that one part of it rested against the base of one of the poles, and one of its ends at a distance of little less than half an inch from the base of the other pole. The spark then passed between the indium and the platinum. The red and blue lines of hydrogen were then observed both by my friend, Mr. G. W. Hemming, Q.C., and myself. Their brilliancy was most markedly increased. This unmistakable indication of the presence of hydrogen, or rather of that form of hydrogen which gives us the *h* line alone *associated into* that form which gives us the blue and red lines, showed us that in the photograph we were not dealing with a physical coincidence, but that in the arc this special form of hydrogen had really been present; that it had come from the indium, and that it had registered itself on the photographic plate, although ordinary hydrogen persistently refuses to do so. Although I was satisfied from former experi-

ments that occluded hydrogen behaves in this respect like ordinary hydrogen, I begged my friend, Mr. W. C. Roberts, F.R.S., chemist to the Mint, to charge a piece of palladium with hydrogen for me. This he at once did, and I take this present opportunity to express my obligation to him. I exhibit to the Society a photograph of this palladium and of indium side by side. It will be seen that one form of hydrogen in indium has distinctly recorded itself on the plate, while that in palladium has not left a trace. I should add that the palladium was kept in a sealed tube till the moment of making the experiment, and that special precautions were taken to prevent the two pieces between which the arc was taken becoming unduly heated.

To sum up, then, the facts with regard to hydrogen: we have *h* differentiated from the other lines by its appearance alone in indium, by its absence during the eclipse in 1875, when the other lines were photographed by its existence as a short line only in the chromosphere of the sun, and by the fact that in the experiments of 1869 a very high temperature was needed to cause it to make its appearance.

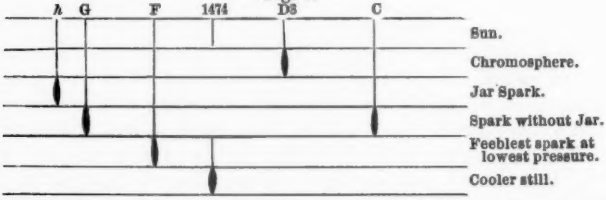
Fig. 6.



With regard to the isolation of the F line I have already referred to other experiments in 1869, in which Dr. Frankland and myself got it alone. I exhibit to the Society a globe containing hydrogen which gives us the F line without either the red or the blue one.

The accompanying drawing (Fig. 6) shows how these lines are integrated in the spectra of the sun.

Fig. 7.



I have other evidence which leads to the conclusion that the substance which gives us the non-reversed line in the chromosphere and the line 1474 of Kirchoff's scale, termed the coronal line, are really other forms of hydrogen. One of these is more

simple than that which gives us *h* alone, the other more complex than that which gives us F alone. The evidence on this point is of such extreme importance to solar physics and throws so much light on star structure generally, that I shall reserve it for a special communication.

In the meantime I content myself by giving a diagram (Fig. 7) in which I have arranged the various groupings of hydrogen as they appear to exist, from the regions of highest to those of lowest temperature in our central luminary.

Summation of the above Series of Facts.

I submit that the facts above recorded are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds.

The other Branches of the Inquiry.

When we pass to the other possible evolutionary processes to which I have before referred, and which I hope to discuss on a future occasion, the inquiry becomes much more complicated by the extreme difficulty of obtaining pure specimens to work with, although I should remark that in the working hypothesis now under discussion the cause of the constant occurrence of the same substance as an impurity in the same connection is not far to seek. I take this opportunity of expressing my obligations to many friends who have put themselves to great trouble in obtaining specimens of pure chemicals for me during the whole continuance of my researches. Among these I must mention Dr. Russell, who has given me many specimens prepared by the lamented Matthiessen, as well as some of cobalt and nickel prepared by himself; Prof. Roscoe, who has supplied me with vanadium and cæsium alum; Mr. Crookes, who has always responded to my call for thallium; Mr. Roberts, chemist to the Mint, who has supplied me with portions of the gold and silver trial plates and some pieces of palladium; Dr. Hugo Müller, who has furnished me with a large supply of electrolitically-deposited copper; Mr. Holtzman, who has provided me with cerium, lanthanum, and didymium prepared by himself; Mr. George Matthay, of the well-known metallurgical firm of Johnson and Matthay, who has provided me magnesium and aluminium of marvellous purity; while to Mr. Valentin, Mr. Mellor, of Salford, and other friends, my thanks are due for other substances.

I have already pointed out that a large portion of the work done in the last four years has consisted in the elimination of the effects of impurities. I am therefore aware of the great necessity for caution in the spectroscopic examination of various

substances. There is, however, a number of bodies which permit of the inquiry into their simple or complex nature being made in such a manner that the presence of impurities will be to a certain extent negligable. I have brought this subject before the Royal Society at its present stage, in the hope that possibly others may be induced to aid inquiry in a region in which the work of one individual is as a drop in the ocean. If there is anything in what I have said, the spectra of all the elementary substances will require to be re-mapped and re-mapped from a new standpoint; further, the arc must replace the spark, and photography must replace the eye. A glance at the red end of the spectrum of almost any substance incandescent in a voltaic arc in a spectroscope of large dispersion, and a glance at the maps prepared by such eminent observers as Huggins and Thalen, who have used the coil, will give an idea of the mass of facts which have yet to be recorded and reduced before much further progress can be made.

In conclusion I would state that only a small part of the work to which I have drawn attention is my own. In some cases I have merely, as it were, codified the work done by other observers in other countries. With reference to that done in my own laboratory, I may here repeat what I have said before on other occasions, that it is largely due to the skill, patience, and untiring zeal of those who have assisted me. The burthen of the final reduction, to which I have before referred, has fallen to Mr. Miller, my present assistant; while the mapping of the positions and intensities of the lines was done by Messrs. Friswell, Meldola, Ord, and Starling, who have successively filled that post.

I have to thank Corporal Ewings, R.E., for preparing the various diagrams which I have submitted to the notice of this Society.

ART. XII.—*On the Velocity of very Loud Sounds;* by WILLIAM W. JACQUES, Fellow of the Johns Hopkins University.

IT is very well known that the velocity of a musical sound is, within very wide limits, sensibly independent of its intensity and of its pitch. The experimental proof of this is that a piece of music, played by a military band at a considerable distance, comes to the ear of the observer with its harmony entirely undisturbed.

A consideration of the theory of the propagation of a musical sound too, shows that for sounds such as we ordinarily hear, in which the change of density from the rarified to the con-

densed portion of the wave is small compared with the density of the undisturbed air, the velocity should be independent both of the intensity and the pitch.

When, however, we come to the consideration of a loud and sharp shock or explosion, in which the disturbances are very violent and abrupt, we cannot be at all sure that the changes of density are negligibly small, and hence that the velocity of sound for such cases would be a constant.

So little is known of the conditions in the case of the formation and propagation of sound from a center of explosion, and the mathematical considerations of such conditions as we may presume are so difficult, that we must look almost entirely to experiment for our knowledge of the propagation of very loud sounds. But our experimental evidence on this point is very limited. Nearly all of the experiments that have been made upon the velocity of sound have been made with a cannon and have not agreed remarkably well with each other; nor have the thermodynamic quantities calculated from them, on the supposition that the velocity is identical with that of a musical sound, agreed very well with the values of the same quantities determined by other methods. But we cannot say whether these errors are due to the character of the sound or to other causes.

The very short interval between the flash and the report of a stroke of lightning, even when it takes place at a considerable distance, has been instanced* as a proof of the greater velocity of very loud sounds, but, so far as the writer is aware, this has not yet been reduced to experiment.

The experiments of Regnault† in water pipes showed that the velocity of a pistol report became slightly less each time that it was reflected along the pipe, but the change was very small and its cause is doubtful.

The following paper contains an account of some automatic measurements of the velocity of sound in the immediate vicinity of a cannon. The results show that the velocity near a cannon is considerably different from that at a distance and point out a considerable error that has been introduced into the most important measurement of this quantity.

The experiments were made at the United States Arsenal in Watertown, Mass.

The method used was an automatic measurement of the velocity at different distances, varying from ten to one hundred and ten feet, from the mouth of the cannon, by means of a series of membranes‡ electrically connected with a chronograph.

In the midst of a large level field was placed a six-pound brass field piece. In the rear of this, at distances of 10, 30, 50,

* Earnshaw, Phil. Mag., 1860. † Regnault's Memoirs.

‡ Regnault used membranes, though unlike these, in his water-pipe experiments.

70, 90 and 110 feet from the mouth of the cannon, were placed the membranes, elevated about three feet above the ground. These membranes consisted each of a hoop nine inches in diameter over which was stretched a sheet of thin rubber. To the center of the membrane, and on the side toward the cannon, was attached a very small shelf of polished brass. Upon this rested one end of a delicate steel spring, the other end being fixed to an independent support.

The wire that brought the current of electricity from the chronograph house was connected with the spring, and from the shelf a second wire returned to the chronograph. When the spring rested upon the shelf the circuit was closed. The passage of the sound wave, however, would move the membrane and break the circuit, causing a register on the chronograph. When the spring fell it rested upon a contact point from which a wire ran to the next membrane of the series, so that the circuit, immediately after being broken at the first membrane, was made again through the second, before the sound wave reached it. In this way the current could be transferred to all the membranes of the series and the successive breakings and makings of contact, as the sound wave passed each one, could be registered on a chronograph placed at a distance.

The chronograph used was of the Schultz form and consisted essentially of a rapidly and uniformly revolving cylinder of silver covered with lamp black, which was made one pole of the secondary coil of an inductorium, the primary coil of which was in circuit with the membranes. The other pole of the secondary coil was a fine metal point brought very near to the surface of the cylinder. When the primary circuit was broken or completed at the membranes, a spark passed between the metal point and the cylinder and made a fine dot in the lamp black. By the side of the point was an electrical tuning-fork which traced a sinuous curve of times on the lamp-blacked surface of the cylinder. The time could thus be measured to ·00001 of a second. All that was necessary then for the experiment was to choose a moment when the air was as nearly as possible at rest and then, the membranes being in order, to start the chronograph and fire the gun. The distances between the membranes were then accurately measured, the times of passage between successive membranes determined from the chronograph and the temperatures read off from thermometers placed at each membrane.

The experiment was many times repeated with the membranes interchanged, with different velocities and parts of the chronograph cylinder and with other precautions to prevent possible errors, but always with the same result. It was found that immediately in the rear of the cannon the

velocity of sound was less than at a distance, but that going further and further from the cannon, *the velocity of sound rose to a maximum considerably above the ordinary velocity and then fell gradually to about the velocity usually received.*

In order to determine whether the first low velocities were due, as was supposed, to the retarding influence of the bodily motion of the air around the cannon, it was pointed at right angles to its first position, when it was found that the maximum velocity came nearer to the cannon. Had the cannon been turned in the direction of the line of membranes the retardation would probably have become an acceleration. The experiment was, however, of course impracticable. That this apparent retardation was not due to the difference in time of action of the membranes, due to a variation of the force of the wave, is evident both from the very slight force required in either case and from the fact that the variation noticed is in the wrong direction.

The charge of powder was considerably varied and the heaviest charges, of course, caused the greatest deviation from the ordinary velocity.

The successive series of experiments, owing to differences in the charge and in the loading, gave different values of the velocity at any one place, but the facts above stated always remained the same.

Accordingly each series represents the condition of things better than the mean of several, and I have here given a table of three of the best series.

The first column represents the distance from the mouth of the cannon; the second the values of the corresponding velocities in the rear of the cannon, when the charge was one and a half pounds; the third when the charge was reduced to half a pound, and the last when the cannon was pointed at right angles to the line of membranes.

	Velocities reduced to 0° C.		
	Rear of cannon.		Side of cannon.
Interval.	1½ lbs.	½ lb.	
10– 30 feet.	1076 feet.	----	----
30– 50	1187	1032	1067
50– 70	1240	1091	1162
70– 90	1267	1120	1201
90–110	1262	1114	1188

The conclusions that we may draw from these experiments are: 1. That the velocity of sound is a function of its intensity. 2. That experiments upon the velocity of sound in which a cannon is used contain an error, probably due to the bodily motion of the air near the cannon. Evidently a musical sound of low intensity must be used for a correct determination of the velocity of sound.

ART. XIII.—*Has Lake Winnipeg discharged through the Minnesota within the last two hundred years?* by J. E. TODD.

AFTER reading General Warren's article in the December number of the Journal, I casually took up a copy of Stansbury's Report on Salt Lake. On page 152 I found a curious account of Baron La Hontan's Expedition up the "Long River" in 1689. The Baron states that he passed down the Wisconsin to the Mississippi, which he ascended nine days, when he "entered the mouth of the Long River, which looks like a lake full of bulrushes," up which he sailed for six weeks, but was prevented by the advance of the season from reaching its source. He learned from the natives, however, considerable about its upper course, which he incorporated in his map.

Captain Stansbury gives a map drawn in 1710 by John Senex, F.R.S., on which the discoveries of La Hontan are given. This represents the "Long River" as being like a long lake toward its source, and flowing nearly due east through 25° of longitude. Another curious point, it is represented as having a continuous water connection through the "Moingona" River with the Mississippi, at a point far south of its proper mouth, where La Hontan entered it.

La Hontan describes it as "all along very slack and easy, abating about three leagues," and the channel unusually straight. "Its banks," he says, "have a dismal prospect, and the water itself has an ugly taste; but then its usefulness atones for such inconveniences, for 'tis navigable with the greatest ease, and will bear barques of fifty tons."

That the "Long River" *may* have been the ancient Minnesota seems indicated, therefore, by their corresponding in position of their mouths, their similarity in breadth of stream and slowness of current. Moreover, the "Moingona" would then correspond to the Blue Earth and Des Moines, whose sources were formerly connected by "Union Slough," as shown by Dr. C. A. White, in his report on the Geology of Iowa.

The curious *eastward* direction of the Long River may possibly be explained partly by the strong eastward declination of the magnetic needle at that time, and partly by the strong desire of the Baron to find a way across the continent. Preconceived notions seem to have influenced him in several other of his statements and conclusions, but perhaps not more so than was customary in those days.

Tabor, Iowa, Dec., 1878.

ART. XIV.—*On the Results of the Spectroscopic Observation of the Solar Eclipse of July 29th, 1878*;* by GEORGE F. BARKER.

To Professor Henry Draper, M.D., Director of the Draper Eclipse Expedition:—

Dear Sir: I beg leave to submit to you herewith my Report on the spectroscopic observations made, and on the results obtained at Rawlins, Wyoming Territory, during the solar eclipse of July 29th, 1878, that portion of the work having been allotted to me by yourself in the organization of the expedition.

The instruments and apparatus used in the observations were loaned for the purpose from the physical cabinet of the University of Pennsylvania. They consisted (1) of an equatorially mounted achromatic telescope of four inches aperture made by Jones of London, (2) a direct-vision astronomical spectroscope by Merz of Munich, (3) a second direct-vision spectroscope by Hoffmann of Paris, (4) and a pocket spectroscope by George Wale & Co. Beside this spectroscopic outfit, a second four-inch achromatic telescope by Dollond was taken for use with the tasimeter by Dr. Edison, and a Savart, a Senarmont, and an Arago polariscope, for determining the polarization of the corona. The Merz spectroscope above mentioned is described in the Philosophical Magazine, IV, xli, Feb. 1871. It is provided with two compound direct-vision prisms, of which one or both can be used at pleasure, each consisting of five single prisms, two of flint glass with a refracting angle of 84°, and three of crown; one of these having a refracting angle of 84°, the others of 87°. The dispersive power of each of these compound prisms is about equal to that of two equilateral prisms of flint glass. The instrument has a collimating and an observing telescope, each furnished with an object glass two-thirds of an inch in aperture and four inches in focal length. The prism-tube is attached to the collimator by two centers, giving it a lateral motion about a line passing through these centers, which constitutes an axis parallel to the slit. The observing telescope is similarly attached to the tube carrying the prism. These motions serve to alter the incidence of the rays upon the surface of the prism, and also to bring any special part of the spectrum into the middle of the field. The observing telescope is provided with a positive eye-piece of an equivalent focal length of one inch, and also with a needle micrometer, having an eye-piece of one-half inch focus. The graduations upon this micrometer are strongly cut, enabling the positions and the distances of the lines measured with it, to be

* Read by permission of Dr. Draper at the St. Louis meeting of the American Association for the Advancement of Science.

easily read even in a faint light. The spectroscope was firmly attached to the draw tube of the equatorial telescope by means of an open frame made by Zentmayer, so that the position of the image with reference to the slit could be readily observed.

The time from the 19th of July, the date of our arrival at Rawlins, until the 29th, was occupied in setting up the instruments, in getting them into adjustment, and in practice with them. It was found that with only one of the compound prisms of the Merz spectroscope, the slit being placed radially, it was easy to observe the lines C and F reversed in the chromosphere, and also the bright line D_3. On the morning of the day of the eclipse, the solar edge was examined for protuberances, in order to locate them in advance of totality. But a single one was noticed, this being on the southwestern edge of the sun. As the time of first contact approached, the spectroscope was removed and a paper screen was attached to the draw tube, an image of the sun being formed on this screen by means of the eye-piece; thus enabling the time of this contact to be approximately determined and the subsequent progress of the eclipse to be conveniently observed. No spots were seen under these circumstances, though this could hardly have been expected since the solar image was so small, scarcely three inches in diameter, unless the spots were of large size. As the time of second contact drew near, the spectroscope was replaced upon the equatorial. Since you deemed it of importance to pay special attention to the oxygen lines in the vicinity of G, the micrometer of this instrument was, at your suggestion, so adjusted that one of its needle points rested on the hydrogen line near G, and the other on the line known as *h*. After the last ray of sunlight had disappeared, I took a few seconds of the precious time to observe the eclipse with the naked eye. The moon appeared intensely black, surrounded by a pinkish halo, extending to about two-fifths of a lunar diameter from the limb, and occupying the entire circumference. At two points this halo was expanded into radial streamers, one of which had parallel sides with a deeply indented or swallow-tailed end, extending westward of the sun and apparently lying in the ecliptic; the other appeared single, was on the eastern edge, and was inclined twenty degrees or more to the north of the ecliptic. The former of these streamers was traced to a distance of about a lunar diameter from the edge, the latter to a somewhat less distance. No structure could be seen in the halo, but in the streamers traces of parallel rays appeared to be present. The amount of light emitted by the corona was a surprise to me. Preparations had been made for using artificial light for reading the circles, but this was found not

to be at all necessary. The amount of light seemed to be nearly or quite equal to that given by the moon when ten days old. No protuberances were seen with the naked eye nor were any streamers observed, other than those already described. A glance at the eclipsed sun was then taken through the finder of the equatorial. The magnifying power being low, the corona presented much the same appearance as to the naked eye. But the streamers showed much more distinct evidences of a radiated structure and a pale rosy protuberance was observed on the southwestern edge of the dark disc. This was undoubtedly the same prominence which was observed previous to totality.

Turning my attention now to the spectroscope, upon the slit of which the coronal image had already been brought by means of the finder, the slit being placed radially, the first glance through the instrument showed me a bright but an absolutely continuous spectrum. The region under examination was of course that portion of the spectrum which had been placed before totality between the needle-points of the micrometer. Totally unprepared for so unexpected a result, I moved the observing telescope so as to bring the green portion of the spectrum into the field, expecting certainly to see 1474 K and by the appearance of this line to determine whether my instrument was out of adjustment; and if it was to adjust it again. But no bright line was there; the green region appeared as continuous as the blue. I then gradually closed the slit—which had been previously adjusted on the solar spectrum so that the line D appeared nebulous on its edges—thinking that I might in this way improve the definition; but with no better result; no bright lines could be seen. To my great surprise however, when the slit was thus narrowed, the region which was then under examination, that extending from *b* to G, appeared filled with dark lines on the brighter background, these dark lines being readily recognized from their general appearance as the solar lines of Fraunhofer. Still intent on getting bright lines, I opened the slit again gradually, moved the observing telescope over the entire length of the spectrum from red to violet, repeating the operation three times and varying the width of the slit from time to time in each region; but not a single bright line could be detected. I then requested you to come and take a glance through my spectroscope, as had been previously agreed; saying that although I could see dark lines and a continuous spectrum, I was unable to detect a single bright line and knew not what to make of it. You were then looking at the eclipse through your ingenious little telespectroscope of two inches aperture. You came to my instrument, looked at the spectrum, moved the observing telescope over its

whole length and remarked that the results in my spectroscope agreed entirely with those in yours, and that the spectrum appeared in both absolutely continuous. My mind being thus relieved, I took my place again at the spectroscope, and this time, placing the slit tangential to the moon's limb, I moved the observing telescope from end to end of the spectrum, opening and closing the slit at intervals; but the spectrum appeared as continuous as before. Again the image was adjusted so that the slit was once more radial; and this time on still a different portion of the corona. On examining again the spectrum, no bright lines appeared, except once for an instant, when the slit passed over the small chromospheric prominence already noticed. Warned by Mrs. Draper's clear and distinct counting that the precious 165 seconds had two-thirds gone, I decided to devote the time still remaining to a more careful observation of the dark Fraunhofer lines. Now, for the first time, as I adjusted the width of the slit and its position on the corona with more care, I observed that these lines did not pass clear across the field, but were of a length corresponding to the width of the coronal image on the slit. At the base of the spectrum, which corresponded to the base of the corona, they appeared bright and sharp; certainly quite as much so as in the light of the moon similarly condensed; though the continuous spectrum which formed their background was relatively brighter than in moonlight. There was no difficulty in identifying them as Fraunhofer lines from their general appearance and position; but some of them could be identified beyond question. Such were *b* and F, which were especially distinct, and D, E and G which were considerably less so. They faded gradually out from the base of the spectrum upward, appearing to end where the continuous spectrum of the corona was limited above. While thus employed, a flash of sunlight told us that totality had ended and that the solar eclipse of 1878 was over.

In discussing the results of the spectroscopic observations which have now been detailed, I am, in the first place, quite at a loss to account for the fact that no bright lines were seen by me, notwithstanding the persistent efforts made to get them. The failure to observe them can be accounted for, as it would seem, only on the ground that with the dispersive power employed, the bright lines were too faint to be seen on the much brighter background of the continuous spectrum.

The lessons to be drawn from these spectroscopic observations appear to be few and simple. The absence of bright lines, or at least of any which were at all brilliant, proves clearly the absence in the solar coronal region of any considerable mass of incandescent gas or vapor, which shining by its own light would of course give a bright-line spectrum. The presence of

Fraunhofer lines in the coronal spectrum shows conclusively the presence of reflected sunlight in the light of the corona and goes to establish the theory, long ago suggested, that masses of meteoric matter raining down upon the solar surface from all directions reflected to us the light of the sun, and were, therefore, the essential cause of the coronal phenomena. And, finally, the fact of the increased brightness of the continuous spectrum, as compared with the intensity of the dark lines of Fraunhofer, goes to strengthen the probability that there is still other light in the corona which comes to us from the incandescent liquid or solid matter of these intensely heated meteoric masses. These conclusions, deduced very simply from my own spectroscopic results, agree completely, I am happy to find, with those drawn from your most excellent photographs, as well as from the ingenious heat-measurements of Dr. Edison and the polariscopic determinations of Dr. Morton.

In concluding this Report, you will permit me I am sure, to express the great gratification which the Draper Eclipse Expedition has continually afforded me. The delight at being able to witness a total eclipse of the sun, has been intensified by the complete success of all the attempts made to observe it, and by the most agreeable companionship during the trip. My obligation to you for the opportunity is as profound as my thanks to you for it are sincere and cordial.

Truly yours, GEORGE F. BARKER.

Philadelphia, August 10th, 1878.

ART. XV.—*On a mode of measuring the Velocity of Sound in Wood;* by MAGNUS C. IHLSENG, Ph.D.

[Read before the National Academy of Science, October, 1877.]

THE subject of this paper was suggested to me by Professor Rood, and the work carried out under his direction in the physical laboratory of Columbia College.

Since the study by Chladni of longitudinal vibration in rods, many researches upon this point have rendered them of great importance for the determination of the velocity of sound and the elasticity of solid bodies. The methods of measuring the velocity of sound in solids, which depended upon the perception by the ear of the unison of two notes were difficult in practice and gave imperfect results. Du Hamel* proposed to cause a pen attached to the rod to describe a wave line on a revolving glass plate, and thus to register vibrations; he did not carry the project into effect. Wertheim† in his excellent work em-

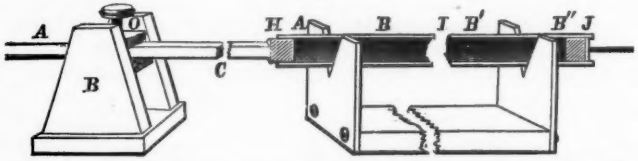
* Journal de Ecole Polytechnic Cohier, xxiii, 19.

† Pogg. Annalen, Ergänz., ii, 1.

ployed a method of this kind, and connected with the rod a chronographic apparatus. He determined the transverse vibrations of the rod by simultaneously registering those of a tuning fork, and then superimposed the longitudinal vibrations of the rod upon its transverse, and ascertained from the corrugated wave line thus produced, the relation between the longitudinal and transverse vibrations.

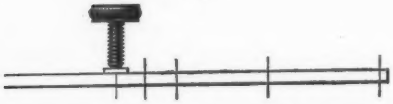
Kundt,* however, in 1866 contrived a method which is so well known as to need only a few words of description. A sounding rod is compelled to set into vibration a column of air enclosed in a glass tube, and in accordance with the laws of the organ pipes, the ratio of the length of the wave in the sounding rod to that of the wave of air in the glass tube, is obtained. Having corrected this for the temperature and the hygrometric state of the atmosphere it is possible to obtain a value for the velocity of propagation. During the past year, Kayser† employed Kundt's method for studying the ratio of the specific heat under a constant volume to the specific heat under a constant pressure.

1.



All of the above experiments were made upon metals. With the exception of MM. Wertheim and Chevandier's extended researches‡ upon the woods of the Vosga forests, I know of but very few determinations of the velocity of sound in wood. Having intended to measure the velocity of sound in some American woods, Kundt's method was first employed; the apparatus was similar to his but somewhat modified to suit the

2.



special circumstances. Fig. 1 shows the arrangement in perspective; B is the vise holding the rod, AC, clamped by a screw, O, acting on a plate of brass which presses the rod firmly upon its rest; the vise is screwed upon the table, which is fastened to the floor and braced to avoid shaking. The rod is rubbed by a resined woolen cloth along A, and being set into vibration,

* Pogg. Ann., cxxvii, 337. † Ibid., 1877, N. 10, p. 218.
‡ Comptes Rendus, xxiii, 663.

communicates it to the air in the glass tube, I, the dust figures then are formed at the nodes A, B, B′, B″, etc. The distances AB and B′ B″ are the semi-wave lengths of air corresponding to the note given by the rod. Although the length AB″ does not materially affect the length of the waves, it has been found best to make it equal to a multiple of the semi-wave lengths as then the figures are most definite. The glass tube was 1^{m}

TABLE I.

Name of wood.	No. rod.	Length.	Section.	Sp. Gr.
Cedar,	1	1836·0mm	14·25 × 21·45mm	·432
"	2	1838·4	14·15 × 21·45	·482
"	3	1838·75	15·6 × 20·45	·465
"	4	1836·72	14·3 × 21·6	·417
"	24	1650·0	14·3 × 21·1	·478
White wood,	5	1876·92	12·4 × 26·35	
"	6	1838·57	11·77 × 26·42	·476
"	7	1834·0	12·35 × 26·85	·443
"	8	1162·25	12·38 × 13·17	
"	9	1162·55	13·0 × 13·14	
"	10	1142·37	12·28 × 13·00	·478
"	11	1164·94	12·79 × 13·23	
"	12	1184·73	12·94 × 13·16	
White pine,	13	1842·6	17·86 × 21·27	·491
" "	15	1841·9	17·45 × 23·05	·432
Yellow "	23	1052·45	12·17 × 24·11	·664
Hickory,	14	1550·5	24·27 × 12·95	·922
White ash,	16	1836·5	9·55 × 19·65	·544
"	17	1838·26	9·55 × 19·70	·541
"	18	1809·7	14·98 × 17·6	·593
White holly,	19	1378·5	10·30 × 10·91	·562
Mahogany,	20	1349·1	11·54 × 11·52	·540
Black Walnut,	21	1378·63	10·82 × 9·73	·518
Cherry,	22	1560·1	10·92 × 11·49	·693
Red oak,	25	1494·7	12·35 × 18·28	·650
White oak,	26	1494·5	12·68 × 17·98	·775

Velocities obtained by Kundt's method.

TABLE II.

Rods.	Number of observations.	Temperature.	Velocity.	Probable error.
No. 1	25	15°·5 C.	3906·3 M.	2·4 M.
" 2	40	20·7	4027·6	8·08
" 3	25	16·1	3966·4	4·62
" 4	25	15·8	4071·68	9·90
" 5	25	13·5	5164·6	2·58
" 6	30	15·6	5129·9	12·29
" 7	35	16·3	5249·3	2·6
" 8	30	14·0	4912·31	4·3
" 9	35	14·4	4739·63	4·3
" 10	30	14·6	4712·53	2·4
" 11	25	14·5	5309·4	7·9
" 12	25	14·3	4845·17	3·4
" 13	35	15·4	4783·15	2·5
" 14	25	16·4	4110·07	2·1

long and 19^{mm} internal diameter; the cork H, inclusive of the two pins which held it to the rod, weighed 1·08 grams. With this apparatus the velocity of sound in the following woods was measured; cedar, white wood, yellow and white pine, hickory, white ash, holly, mahogany, black walnut, cherry and white oak.

The probable error is so small that it would seem to indicate either that the procedure is a very accurate one or that the error is constant and in the same direction.

But during the progress of these experiments, another method was devised whereby the rod registered its own vibrations. A blackened glass plate was drawn rapidly by in a horizontal direction by a falling weight, and matters were so arranged that a pen attached to the rod of wood wrote its vibration upon the plate, which simultaneously registered those of a tuning fork. The apparatus was capable of being leveled accurately, and it had connected with it an arrangement that enabled the observer to set it into action at any desired instant. The final result, of course, is that two parallel sets of curves are traced on the smoked glass. For the purpose of making the measurements it was necessary to compare the corresponding portions of the two curves, and two lines were drawn at right angles to them so as to include corresponding portions. This was effected by a little piece of apparatus especially contrived for the purpose. The number of waves registered were then counted with the aid of a compound microscope, which with its micrometer eye-piece was also used to measure the fractional parts of the waves. It was found possible with this arrangement to estimate, without any difficulty, the fiftieth part of a wave length. The following columns give ten readings of a single wave length, showing their agreement; the wave traced on the smoked glass, in this case, had an average length of 3·56^{mm}.

3·57	3·58
3·60	3·55
3·58	3·42
3·70	3·30
3·52	
3·75	3·56

For determining the rate of the fork, the average of six determinations of 5·033 seconds each, by a chronograph, was taken as follows: $\frac{7701{\cdot}70}{6\times 5{\cdot}033}=255{\cdot}04$.

The difference between this quantity and the number of vibrations stamped on the fork by Kœnig (256) was due to the fact that the fork was loaded by the wax and pointer.

TABLE III.

Rods.	Kundt's Method.		Graphic Method.		Difference.
	Velocity V.	Probable Error.	Velocity V'.	Probable Error.	
1	3849·28 M.	2·5 M.	3785·3 M.	5·1 M.	64·0 M.
2	4061·56	2·79	4024.3	5·62	37·26
3	3934·49	4·35	3870.5	5·51	64·0
6	4985·05	10·30	4896.7	9·56	88·35
7	5278·59	9·69	5225.3	3·37	53·29
13	4753·02	2·88	4715.3	5·45	37·72
23	4326·36	2·14	4265.6	7·88	60·76
24	3950·90	3·66	3871.2	6·64	79·7

The first determinations were on the comparison between the graphic and Kundt's method. To this end an experiment was made by combining both methods, thus: while the rod was urging the air column into synchronous action by means of the cork (Kundt's method), it simultaneously registered, with its pen, the rate of its vibration. The results obtained by this double method should, of course, agree, as both the motions originate from the same source. They were found, however, to differ as shown in the annexed table.

These values are averages of fifteen observations. A glance at this table shows that the air method gives results which are invariably higher than the graphic method, the difference between the numbers being much larger than the sum of the errors in the two cases. This may be due to the fact of Kundt using too large a coëfficient for the correction for moisture and temperature. The experiments having been conducted during the winter, the amount of moisture in the laboratory was a minimum. Thus, calculating the velocities on the supposition that the air was perfectly dry we obtain the following results which are still too high.

Rods.	Calculated Velocity.	Diff. from Graphic Method.
1	3838·04 M.	+ 53·74 M.
2	4038·42	+ 14·12
3	3922·53	+ 52·03
6	4966·34	+ 69·64
7	5263·93	+ 38·63
13	4728·40	+ 13·10
23	4311·05	+ 45·45
24	3939·01	+ 67·81

These comparisons show, it would seem, that the tendency of Kundt's methods, at least as used by me, was to give results which were somewhat higher than they should have been.

To the above it may be added that in these experiments on Kundt's method, the stopper J, figure 1, was placed at a node, thus making an air column in the tube which was in unison

with the rod; the resistance to the rod's vibration was then small. But if we remove the stopper from the node and place it in the middle of a ventral segment, the resistance to vibration becomes a maximum, the rate of the rod is diminished and we have a lower number of vibrations actually executed and registered by the rod. Rod No. 23 was subjected to this test, its length being 1051·15^{mm}. While the rod was urging the air column into vibration, the stopper being at a node, the rate of vibration registered by the rod itself was 2029 per second. Removing the stopper to a ventral segment diminished the rate to 2009·2 vibrations. In the latter determination no satisfactory dust figures could be obtained.

Having thus compared these two methods, we now proceed to give the results obtained by the graphic method upon the several rods. The rods were clamped in the center and without any load except the inappreciable weight of the pen.

TABLE IV.

Velocity of sound in American woods: measured by the Graphic Method.

Names of Wood.		Length.	Sp. Gr.	Temp.	Velocity.	Probable Error.
Cedar,	No. 1,	1836· mm	0·432	21·5 C.	3797·2 M.	3·56 M.
"	2,	1838·4	·482	20·9	4073·89	9·99
"	3,	1838·75	·465	28·4	3864·79	5·00
"	4,	1836·72	·417	---	4161·65	5·62
"	24,	1650·	·478	18·8	3915·47	5·89
White Wood,	6,	1838·57	·476	22·4	4927·30	7·43
" "	7,	1834·0	·443	19·8	5201·22	8·73
" "	10,	1142·37	·478	18·0	4650·60	3·90
" Pine,	13,	1842·6	·490	20·0	4713·36	6·63
" "	15,	1841·9	·432	20·0	4522·46	7·51
" Ash,	16,	1836·5	·544	27·0	4282·45	5·52
" "	17,	1838·26	·541	27·3	4261·51	5·88
" Holly,	19,	1378·5	·562	21·2	3657·41	4·95
Mahogany,	20,	1349·1	·540	20·0	4135·26	5·39
Black Walnut,	21,	1378·63	·518	19·1	4780·72	5·60
Cherry,	22,	1560·1	·693	18·0	4409·54	5·49
Yellow Pine,	23,	1052·45	·664	20·2	4274·48	5·60
Red Oak,	25,	1494·7	·650	25·0	4179·80	5·34
White Oak,	26,	1494·5	·775	25·0	4316·50	4·87

There were, however, certain difficulties to contend with which will now be briefly mentioned. I had not long been engaged upon this work, when I discovered a tendency to vertical transverse vibration in some of the rods, thus carrying the pointer off the plate. To meet this difficulty a kind of holder was contrived and covered with felt which, while only touching the rod lightly, held it in position. This damper was placed in various positions along the rod, and the corresponding rates of vibration measured. These values were tabulated for the several positions shown in fig. 2, which were as follows: E, as near the end as possible; D, 400^{mm} from E; C, 700^{mm}

from E; B, as near the center as the clamp would allow. The resulting values given in the following table are averages of fifteen observations.

TABLE V.

Showing the effect of the Damper.

Rods.	Damper at end.	At D.	At C.	At B.
No. 1,	3798·9 M.	3777·1 M.	3787·9 M.	3786·4 M.
" 2,	4063·7	4078·5	4062·7	4067·9
" 3,	3883·1	3872·0	3869·1	3865·0
" 6,	4876·8	4895·7	4840·7	4866·7
" 7,	5226·9	5205·0	5208·2	5213·7
" 13,	-----	4511·4	4517·6	4560·1
" 16,	4272·4	4244·4	4255·5	4259·3
" 17,	4259·1	4248·0	4256·0	4265·7

The results given in tables 2, 3 and 4 were obtained without the use of the damper.

It may be worth while to mention that some of the rods while vibrating longitudinally were capable of generating two notes, differing from each other by about a terz. These seemed to depend either upon some inherent condition of the wood or perhaps upon the relation between the two thicknesses and the length of the rod. A little practice is requisite for bringing out the lower note with sufficient power to cause it to be recognized and registered; it also required the exertion of a greater force for its production and always preceded the high note. I also found that the damper affected the high and low notes correspondingly. The velocities corresponding to the high note have already been given in tables 3, 4 and 5, while in table VI are the velocities deduced from the employment of the low note with rods 2 and 3 the damper being used.

TABLE VI.

Rods.	Damper at E.	At D.	At C.	At B.
No 2,	3637·6 M.	3663·2 M.	3663·6 M.	3704·7 M.
No. 3,	3643·87	3696·42	3677·12	3679·6

Subsequent investigation led me to the supposition of the coëxistence of transverse and longitudinal vibrations; the higher note being the normal note produced by longitudinal vibrations, the lower note being due to transverse motion.

In order to test this matter, the glass plate was caused by the falling weight to move in the direction of the length of the rod, and it was found in point of fact, possible in this manner to register its transverse vibration. The number of transverse vibrations obtained for the high note were very nearly equal to the number of the longitudinal vibrations. The same relation existed between the transverse and longitudinal vibrations

producing the low note. This is evident from the table given below:

TABLE VII.

Number of longitudinal and transverse vibrations corresponding to the high note.

Rods	Number of longitudinal vibrations in a second = N.	Number of transverse vibrations in a second = N′.	Difference.
No. 2,	1107·97	1068·29	38·68
" 13,	1227·21	1246·05	18·84
" 19,	1326·59	1333·75	7·16
" 20,	1532·60	------	----
" 21,	1733·89	1717·43	16·46
" 22,	1413·26	1391·80	11·46

TABLE VIII.

Number of longitudinal and transverse vibrations corresponding to the low note.

Rod.	Longitudinal = n.	Transverse = n'.	Difference.
No. 2,	1009·29	978·58	30·71
" 13,	998·58	976·14	22·44
" 19,	1167·62	1162·54	5·08
" 20,	1204·31	------	----
" 21,	1332·04	1211·71	120·33
" 22,	1115·66	1119·24	3·58

The only difference in the appearance of the longitudinal and transverse tracings of either of the notes was in their amplitudes. The high note possessed a larger amplitude in longitudinal vibration, the low note in transverse. If now we compare Tables VII and VIII, we find no harmonic interval embraced in the ratios between N and n, or N′ and n', as is shown below; they however, in some cases approximate to a terz. Terz = 4 : 3.

TABLE IX.

Rod.	Ratio $\frac{n}{N}$.	Ratio $\frac{n'}{N'}$.	
No. 2,	1 : 0·9109	1 : 0·9161	secund = ·888.
" 13,	0·8137	0·7834	terz = ·8.
" 19,	0·8802	0·8701	secund = ·888.
" 20,	0·7858	-----	
" 21,	0·7682	0·7055	quart = ·75.
" 22,	0·7894	0·8041	terz ·8.

From the above experiments, we conclude; (1.) That it is possible to measure the velocity of sound in rods with considerable accuracy by the graphic method; (2.) Kundt's method gives results which are slightly higher than those by the graphic method; (3.) The graphic method demonstrates the existence of transverse along with longitudinal vibration and gives their ratios.

Columbia College, Oct. 25th, 1877.

ART. XVI.—*The Relation of Secular Rock-disintegration to Læss, Glacial Drift and Rock Basins*;* by RAPHAEL PUMPELLY.

THE recent volume of Baron Richthofen's great work on China and Central Asia, is, to a considerable extent, occupied with a description of the great læss formation and of the processes to which it owes its origin.

This remarkable formation covers several hundred thousand square miles in northern China, and larger areas in the rest of Asia. It forms the soil also over an immense area in the western United States. Its thickness varies, in China up to 2000 feet, and to 150 and 200 feet in Europe and America.

Læss is a calcareous loam. It is easily crushed in the hand to an almost impalpable powder, and yet its consistency is such that it will support itself for many years in vertical cliffs 200 feet high. A close examination shows that it is filled with tubular pores branching downward like rootlets, and that these tubes are lined with carbonate of lime. It is to these that it owes its consistency and its vertical internal structure. It is wholly unstratified, and often where erosion has cut into it, whether one foot or one hundred yards, the walls are absolutely vertical. Its vertical internal structure causes it to break off in any vertical plane but in no other. Hence when a cliff is undermined the læss breaks off in immense vertical plates leaving again a perpendicular wall.

It is divided into beds varying in thickness from one foot to two or three hundred which thin out to nothing at the borders and are separated by parting planes. These planes are marked by angular debris near the mountains, and by elongated upright calcareous concretions elsewhere.

This remarkable combination of softness with great strength and stability of exposed surfaces is of inestimable value in a woodless country. In Asia thousands of villages are excavated in the most systematic manner at the base of cliffs of læss. Doors and windows pierced through the natural front give light and air to suites of rooms which are separated by natural walls, and plastered with a cement made from the læss concretions. These are the comfortable dwellings of many millions of Chinese farmers, and correspond to the ruder "dug-outs" of Nebraska.

To the same qualities is due the fact that the læss districts of China are exceedingly fertile plains, in each of which a rapidly progressing erosion has excavated the most labyrinthine valley systems, in which all the members, down to the smallest tributaries, are sunk with vertical walls to depths of from one

* Read before the National Academy of Science, April 10th, 1878.

hundred to several hundred feet. Even the wagon roads become, in time, depressed to a depth of fifty feet and more by the removal of the dust by wind.

There is one more peculiarity of the lœss—it not only is wholly unstratified, but it contains the remains of only land animals, and especially of land snails. Alexander Braun examined 211,968 specimens of shells from the lœss of the Rhine between Basel and Bonn, and found that all were land snails except only thirty-three individuals, consisting of *Limnœa Planorbis* and *Vitrina*, which came from three isolated points in the valley of the Rhine and Neckar.

The lœss, first well known in the valley of the Rhine and in France, was recognized later in other parts of Europe and in the Mississippi Valley. It was always looked upon as a sub-aqueous formation. Fourteen years ago I observed and afterward described some of the great lœss-basins along the boundaries between China and Central Asia. I was lead to the conclusion, chiefly from topographical reasons, that the lœss of these valleys had been deposited in a series of great lakes; and subsequent observers took the same view. But Richthofen, extending his fruitful journeyings over a wide area, found the lœss occupying the loftiest passes of northeastern China, over 8,000 feet above the sea, and he proves, in the most conclusive manner, that an aqueous origin is impossible. For the former theories of lœss formation which required inconceivable conditions and are full of contradictions, Richthofen substitutes an exceedingly simple and thoroughly consistent explanation. I hope I may be excused for giving it in brief terms:

Whenever, from any cause, the winds blowing toward an interior portion of a continent are drained of their moisture on the way, as by the elevation in their path of lofty condensing mountains, the region thus deprived of its rain-bringing clouds soon has its evaporation in excess of its rainfall. Its streams dry up; its soluble and insoluble products of disintegration are no longer carried to the ocean; the region becomes what Richthofen calls a central area, in contrast to peripheral regions which are drained directly into the ocean. The destruction of the vegetation lays bare the surface, and the products of disintegration are blown and sorted by the wind and washed by the occasional rains from the hills down into the valleys. This material is very nutritive and supports the grass of the steppes; the dust left by the winds and the hill-wash are arrested by the grass which they gradually bury while forming the soil for new growths. In this way, portions of the country become buried in their own and their neighbor's debris. Great thicknesses thus gradually accumulate undergoing a transformation into lœss by the rootlets and stems of the vegetation. I will

remark here that Richthofen has made known the fact that the lœss is the most fertile of soils, and that the grain-regions of northern China which have been continuously cultivated for more than 4,000 years require practically no manure but that they are self-fertilizing. This he ascribes partly to the porosity of the material which causes it to absorb carbonic acid and ammonia in large amounts from the air; but more especially to the elevation of nutritive salts in the capillary tubes by diffusion whenever a rain establishes a moist communication between the surface and the saline water below the drainage level.

Thus, whenever climatal changes have restored more or less moisture to the atmosphere of a lœss district, we have in it the utmost fertility. The cold and elevated regions of northern China are the granary of the empire—and the seat of the present great famine.

Attention has been recently called by the German geologists to the great fertility of the European lœss, and Professor Hayden has emphasized the fact that the abundant productiveness of much of the West is due to the same soil.

Recognizing from personal observation the full identity of character of the lœss of northern China, Europe and the Missouri Valley, I am obliged to reject my own explanation of the origin of the Chinese deposits, and to believe with Richthofen that the true lœss, wherever it occurs, is a sub-aerial deposit, formed in a dry central region, and that it owes its structure to the formative influence of a steppe vegetation.

The one weak point of Richthofen's theory is in the evident inadequacy of the current disintegration as a source of material. When we consider the immense area covered by lœss to depths varying from 50 to 2,000 feet, and the fact that this is only the very finest portion of the product of rock-destruction, and again that the accumulation represents only a very short period of time, geologically speaking, surely we must seek a more fertile source of supply than is furnished by the current decomposition of rock surface.

It seems to me that there are two important sources: I. The silt brought by rivers, many of them fed by the products of glacial attrition flowing from the mountains into the central region. Where the streams sink away, or where the lakes which receive them have dried up, the finer products of the erosion of a large territory are left to be removed in dust storms.

II. The second, and I believe, the more important source is in the residuary products of a secular disintegration which we will now consider.

In all regions where the soil is protected by a luxuriant vegetation the greater part of the insoluble products of disintegra-

tion remains *in situ*. Considerable portions of the continents have remained above water during long geological periods. Where this has been the case, and where the region thus exposed enjoyed a peripheral climate with a protecting vegetation and abundant generation of carbonic acid, the feldspathic rocks have been profoundly affected; granite and gneisses being decomposed often to the depth of several hundred feet. In regions underlaid by impure limestones of great thickness, a long continued existence as dry land results in the removal of the lime carbonate and the formation of a residuary accumulation of the insoluble impurities. Thus in Missouri, in the Ozark Mountains, the secular dissolving away of the limestones which contained from two per cent to nine per cent of insoluble silica and clay has left such residuary deposits 20 to 120 feet thick.

The decay of a rock mass progresses from the joints and cracks, by which the mass is divided into polygonal blocks, and the rate of this progress varies, in different rocks, with the nature of the mineral constituents and the physical condition of structure, texture, mineral combination, etc.

In granular rocks, as granites, syenytes, gneisses, granulytes, diorytes, etc., the product is an argillaceous sand or a sandy clay; porphyries, basaltic, trachytic rocks, generally produce ferruginous clays; impure limestones and dolomites undergo the greatest shrinkage of volume, leaving masses of sandy clay with chert separating the broken up representatives of such layers of shales and sandstones as were interstratified with them. Calcareous sandstones and shales also leave sands and clays.

The decay may extend to great depths before it reaches the cores of the larger polygonal blocks. Where this is the case, the disintegrated mass consists of the rounded cores of the blocks surrounded by the decomposition product of the rest of the mass.

Other things being equal, the granular rocks of a region will be nearly or quite reduced to a loose mass when the compact rocks like porphyries and basalts have still a large proportion of their mass represented by the rounded cores.

Even slate rocks—especially talcose, chloritic, hydro-mica and sericite slates—are often affected to considerable depths.

As different rocks are affected in very different degrees by this change, it happens that in regions underlaid by a diversity of rocks, the plane marking the boundary between disintegrated rock and still hard rock must be an exceedingly irregular one.

If we could imagine the loose altered rock removed where this process has been active in depth, the surface exposed would present a remarkable topography, one in which the

hardness of the material would play no causative part. The prominent features, such as ridges and hills, would consist of the rocks that are most resistant to carbonic acid, as for instance, the soft clay slates and mica schists, as well as the hard quartzytes and sandstones. The action of frost does not concern us here, for it is comparatively a surface phenomenon.

The depressions would represent rocks more or less easily acted upon by carbonic acid, water and free oxygen, and to a greater or less extent, all rocks carrying a feldspar in abundance. We need not expect to find here inequalities of surface of the types produced by erosion. The depressions, instead of being valley systems caused by erosion by gravitating material, are necessarily, to a great extent, closed basins produced by the downward growth of the rock decay. Deep and shallow basins without outlets may be formed by the decay of the rock occupying the meshes of a network of less affected dykes or veins; but generally they owe their origin to the unequal distribution of the sources of dissolving reagents on the surface above, for instance, swamps and lakes, and to inequalities in the character of the rock itself. On the other hand, dykes more easily affected than the enclosing rock, and fissures causing the decay of their walls, would both give rise to remarkable defiles. These conditions are all presented in the most striking manner on the plateau of Central Asia.*

* The importance of this decay in depth under protection from erosion seems to have wholly escaped the attention of geologists as a factor of general significance. Even the more remarkable local instances are rarely mentioned and only one or two have found references in text books.

Mr. Darwin states that the numerous travelers in Brazil have all been surprised at the depth to which the gneiss and other granitic rocks as well as the talcose slates in the interior have been decomposed.

Near Rio every mineral except the quartz has been completely softened, in some places to depths little less than one hundred feet.—Darwin, Geological Observations, Part III, p. 143. London, 1851.

Professor J. D. Whitney explains the accumulation, sometimes thirty feet thick of unstratified red clay, chert and galena in the Wisconsin lead region, by supposing it to be the residuum left by the gradual dissolving away of several hundred feet of impure limestone.—Geology of Wisconsin, vol. i, p. 121. 1853.

I have since observed and described the results of this process on a much larger scale among the dolomites of southern Missouri.—Geological survey of Missouri; Iron Ores and Coal Fields, p. 8. 1873.

According to Dr. Benza, the Neilgherry Hills, occupying an area of about 700 square miles in southern Hindostan are of granitic and hornblendic rock and are disintegrated often to a depth of forty feet.—Leonhard u. Brown, Neues Jahrb. für Min. Geol. u. Petrofact., 1838, p. 713.

The memoirs of the Geological Survey of India record the prevalence of this deep-seated decay in the crystalline rocks of that part of the world.

Dr. T. Sterry Hunt, who considers this decay to have taken place in Archæan time, after affirming its prevalence and depths in the southeastern States, sees in it the source of the extensive deposits of hydrated iron ores that occur along the base of the Blue Ridge. He also ascribes its removal in the northern country to the action of long-continued erosion and the final washing away by water and ice during the Glacial epoch.—Proceedings Bost. Soc. of Nat. Hist., vol. xvi, Oct. 15th, 1873.

As soon as we pass the limits of glaciation we find in southern Pennsylvania, Maryland and Virginia the crystalline rocks profoundly affected, especially the feldspathic kinds, but the decay extends also, with differences in kind and degree to the various slates. It may be well seen in the railway cuttings in gneiss between Philadelphia and Baltimore, and in the slates of Frederick County, Maryland.

In the report of the geological survey of Missouri presented in April, 1873, I have called attention to the extent of this decomposition in the granites, porphyries, and limestones of Missouri, and to the great economic importance of its resulting accumulations which I have called residuary deposits in connection with the iron, lead and zinc ores.

In October of the same year, Dr. T. Sterry Hunt described the prevalence of this same decay throughout the southeastern States, often to a depth of one hundred feet or more, considering it to date from a very early geological time.

Over a large part of Europe and America this accumulation has been removed by glacial action. We may assume that during the gradual approach of the glacial epoch the ground in the northern half of the northern temperate zone, as the mean annual temperature fell below the freezing point, became perpetually frozen and when covered by the glacial ice, the thickness of the glacier instead of being measured from the upper surface down to the former soil should be measured down to the bottom of the residuary mass of disintegrated rock. It seems to me that only on this supposition can we explain the enormous amount of the ground moraine that covers our northern country, and the predominance in it of the debris of local rocks. The whole of this loose material must have participated in the movement of the ice, though far more slowly than the overlying and more nearly pure ice. Thus at any given point, the material on and near the surface would represent localities considerably farther northward than that at the bottom. We can only thus account for the disappearance of the vast amount of residuary disintegrated material that must have existed over the surface of the Archæan feldspathic rocks of eastern British America. But in Northern Asia, north of the 40th degree of latitude there are no traces of a general glacial action such as existed in Northeastern America and Northern Europe. The

According to a verbal communication of Professor W. B. Rogers, this deep-reaching decay is general throughout the schists of the Southeastern States, where its depth often exceeds two hundred feet.

Mr. Alexander Agassiz and Mr. Thomas MacFarlane have both informed me that, at least the granitic rocks of the Andes are similarly affected.

Since this paper was written, I have had occasion to observe the immense extent of the disintegration in place that exists in the slates, diorites and syenitic granites of the foot-hills of the Sierra Nevada in California.

evidence indeed is all the other way. And yet, while the rocks of Southern Asia show extensive residua of disintegration, the results of a secular decomposition protected from erosion by an abundant vegetation, the feldspathic rocks of Northern China and of Central Asia are as free from this as are those of north-eastern America.

The only answer to the question, what has become of them? is, that they have been blown and sifted and assorted by the winds, the heavier fragments remaining to be reduced by weathering and to form the stony steppes, the sand drifting in billowy waves over the country, and forming sand-deserts, while the fine dust floating in the air, an impalpable powder is deposited far and near, and, under the influence and protection of the steppe grasses, is transformed into the lœss.*

It is not many years since the glacial "till" was recognized as an extensive formation. The great development of the lœss has only just been demonstrated by the observations and generalizations of Baron Richthofen. The importance of each of these deposits seems to me to necessitate a recognition of the importance of the great residuary deposits as the source of both the others.

We have here suggestions that bear on many and varied geological problems, some of these which now occur to me I may be permitted to mention.

There are few problems in dynamical geology that have been considered more difficult to solve than the origin of rock-basins. Wherever my route between the great wall of China and the Siberian frontier lay through a region of crystalline rocks, I found that one of the characteristic features of the surface was the prevalence of basin-shaped depressions of all sizes hollowed

* No one can realize the capacity of wind as a transporter of fine material, who has not lived through at least one great storm on a desert. In such a simoon the atmosphere is filled with a driving mass of dust and sand which hides the country under a mantle of impenetrable darkness, and penetrates every fabric; it often destroys life by suffocation, and leaves in places a deposit several feet deep.

The prevailing westerly wind carrying sand, carves and polishes the rocky crest of the Sierra Nevadas, and, as Mr. King tells me, has formed long wind-stream deltas, if I may coin the term, in the form of lofty sand ranges stretching, from each pass eastward, far out on the desert.

The often-cited instances of far-driven volcanic ashes show the ability of the wind to carry comparatively coarse dust through distances of several hundred miles, but it does not seem improbable that the finer particles may remain suspended while the wind makes a complete circuit of the globe. I witnessed on March 31st and April 1st, in 1863, a dust fog at Nagasaki, which lasted two days, and left only a just perceptible film of dust, observable only on the white newly-painted deck of a yacht. A similar fog occurred simultaneously at Shanghae, and both were cotemporaneous with a terrific dust storm which during two days shrouded the country about Tientsin in deep darkness.

I am indebted again to Mr. Clarence King, for the statement that dust fogs occur on the coast of California with the prevailing west wind; and this may be as he suggests, the finest residuum of the lœss dust of an Asiatic dust storm.

out of the rock.* In other words, if filled with water they would have been lakes without outlets and with unbroken sides of rock. In some exceptional instances it was clear that systems of intersecting dykes had been less acted upon by the basin-making process than the intervening rocks, and the basins were formed in these last.

The rock basins of Scotland have been graphically described by Geikie, and while a great many of the countless lakes and lakelets in the region of the crystalline rocks of North America and Northern Europe are valleys of erosion with dams of glacial moraine material, vast numbers of them undoubtedly fill rock-basins. An ingenious explanation of the formation of these rock-bound depressions given by Ramsay and accepted by Geikie as the only reasonable hypothesis, is that the rock-basins of the loch and fiord kinds are due to unequal sculpturing by glaciers.

He says that where the bed of a glacier diminishes its angle of slope, the vertical pressure of the ice on its bed becomes greater, both owing to the lessened inclination and to the increased thickness of the ice, while farther down the incline the widening of the valley causes the ice to spread and consequently to diminish its thickness and pressure; the result being a rock-basin dammed by a rock-bar.

While this may be admitted, with some reservations, as a satisfactory explanation for many fiords and lochs cut in the declivities of a mountain range or of a steep coast, it is useless in regard to the lake-basins of flat countries, like Finland and British America. Geikie endeavors to use this hypothesis to explain also the rock-basins observed by me in Central Asia;† but it is still more useless here as an explanation, because there are absolutely no traces of glaciation in Central Asia, outside of the high mountain chains. These Asiatic depressions are rough and ragged, and the debris contained in them consists of ragged angular fragments of the local rocks, while the glaciated basins of America and Europe are smoothed and polished, and the debris they contain consists of the rounded and scored material of the drift.

The basins of Asia were emptied by wind, and those of Northern Europe and America were emptied by ice, but the wind and the ice were only immediate agents employed in rapidly emptying basins which had been long forming by a process common to both—the secular decay of the rock. I would thus seek the real cause, not in the unequal nature and action of the instrument, but in the unequal physical condition of the

* Pumpelly, Geological Researches in China, Mongolia and Japan, pages 72, 73, 26, Smithsonian Inst., 1866.

† Geikie—Great Ice Age, pages 351–352, D. Appleton & Co., 1875.

material operated on. It is evident, I think, that we have in these residua an adequate source of material for the glacial drift and for the lœss, although other sources have probably contributed in some regions to the formation of the latter, as for instance, rivers which, fed by silt-loaded glacier-streams, sink away on the plains of the central area, leaving their suspended material to dry and drift on the surface.

In reply to some questions bearing on the possible sources of the lœss of the Missouri valley, Mr. Clarence King kindly informs me that during the Pliocene the entire surface of the great plains was covered by a sheet of water which he has called Cheyenne Lake. Its deposits, known as the Missouri Pliocene—2,000 feet thick at the western edge along the Rocky Mountains—become very thin at the eastern margin in Kansas and Nebraska. Throughout the eastern portion at all points over 150 miles east of the mountains, the material is exceedingly fine—so fine that Professor Brewer found that when shaken with water in a test tube it does not settle after standing a year. Furthermore, Mr. King (anticipating one of the generalizations of the forthcoming volume of his reports), says that there was a period of intense dryness affecting the whole region of the great basin, and at least a portion of the great plains. During this period there were exposed to wind-erosion, not only the surface of the plains, but the entire region east and west of the mountains that had been subjected to the Bad-land erosion. The countless cliffs and rounded outlines sculptured out of the soft Tertiary clays and marls which form the Bad-lands were particularly susceptible to wind-erosion. Finally, Mr. King summarizes his answer in these words: "The lacustrine basins of the plains and Rocky Mountains certainly afford sufficient fine material, the period of desiccation between the two floods of the Quaternary affords the necessary dryness, and the prevalent west wind would account for the transportation to the east."

In the warmly contested question as to the capacity of continental glaciers in excavating solid rock, the great extent of the "till" and of the modified drift have been adduced as an evidence of this power. It seems to me rather that the abundance of the drift material may be equally well taken as in great part but not wholly a measure of the effect of long-continued and undisturbed disintegration and of the transporting power of ice, and that a period of glaciation may be, as Rutimeyer insists, a period of comparative rest as regards excavation of hard rock.

The great source from which mountain glaciers derive their moraines is, as is well known, in the incessant rending action of cold—of constantly alternating contraction and expansion—

on the rocks of the peaks and precipices that rise above the ice. But a continental glacier, such as covered Northeastern America, had no such source of supply; for all the mountains were covered with ice and were thus protected from the operation of this process.

The great ice-sheet removed the residuary accumulation of loose material down to the solid rock; in doing this the loose material was ground together, producing much clay and sand; and besides this, the glacier grinding with this material under the immense weight of the ice on the solid rock, exerted an abrading and polishing action, the product of which must have been wholly very fine flour. I would thus measure the excavating action of the continental glacier in solid rock only by a small portion of the finest part of the glacial debris. For, since all the bowlders of the ground moraine have been abraded over their whole surfaces, the amounts of the product of abrasion from the loose residuary material on one hand and from the solid rock bottom on the other should stand to each other in nearly the ratios of their respective surface areas.

Not the least important suggestion is the influence of the causes we have considered on the succession of marine strata. Great areas like Southern China and the southeastern United States have remained as dry land from the Triassic period till now. Throughout this long lapse of time, disintegration and decomposition have been at work, and one has only to look at the excavations in Philadelphia and on the railroads running south from there to get some idea of what these agencies can accomplish. But the abundant vegetation of a moist, peripheral region has so thoroughly protected the residuary mass from erosion that the streams run clear, carrying to the ocean only soluble salts and the comparatively unimportant products of attrition. There are three methods by which these vast stores of slowly-prepared detritus can become available to form mechanical sediments, and all are rapid, in this sense, viz: that other things being equal, by each method, the ocean receives in a given time an enormously greater amount of sediment than it would receive had there been no such accumulation ready at hand. The *first* method is an encroaching sea-coast. The *second* is the removal of the residua as the ground-moraine of a continental glacier to furnish the ocean-border with enormous quantities of gravel, sand and clay, both from the frontal moraine and through the immense influx of debris brought by the rivers, at first from the melting ice, and, for a long time afterward, from the accumulated masses of gravel and sand brought down by freshets. The *third* method is through the mediation of the loess formation. Let us imagine a region of granites, gneisses and impure limestones that has long been

dry land, and has been covered by abundant vegetation, to be transformed into a dry, central area. The destruction of the protecting plants would expose to the fierce winds the loose residua of long periods of disintegration of the rocks. Under this process of a dry separation the material would be sorted, the finest carried to great distances, would find resting places only where under the protection of steppe grasses it could be fixed and transformed into lœss. The coarsest would remain to form the gravelly steppes, while the material of intermediate grain, chiefly siliceous sand, not capable of giving sustenance nor of being fixed, would drift in dune-like waves before the prevailing winds, transforming everything in its track into desert wastes.

We have in the immense lœss deposits of China, often more than one thousand feet thick, and in those of Europe and west of the Mississippi, which are often measured by hundreds of feet, the best evidence of the ability of the lœss-forming process to accumulate vast masses of fine material in a comparatively short time.

Let us now imagine this same region to be subjected to a reversal of this process, and transformed again into a transition zone, in the manner so strikingly described by Richthofen. As the rainfall gradually comes to balance and exceed the evaporation, the streams that empty into the ocean push their sources farther and farther back into the lœss territory, gradually tapping the Salt Lakes and taking up their tributaries until the ocean again drains the continent.

This is the period of the turbid waters. The rapid growth of systems of drainage and erosion in a lœss territory hardly admits of description. It is limited only by the amount of rain-fall. Coasts which during geological ages have received only limpid streams, and in whose waters the corals and lower organisms have worked heretofore, scarcely disturbed by the products of wave-erosion, are now continuously deluged with yellow silt.

The great silt-loaded rivers of the world are, in at least nearly all instances, streams which after running clear water for long ages, are now merely temporarily employed in making a rapid transfer of the lœss which is itself the product of a concentration by wind of the products of a slow disintegration which may have lasted from the Carboniferous period to the Post Tertiary.

When we consider not only the enormous volume of this silt, but also the fact that its extreme fineness causes it to be carried over great areas in suspension before it sinks, we can conceive that its effects may be catastrophic as regards the less plastic forms of life. This would be the case especially where

the range of a fauna peculiar to a given coast is more limited than the area affected by the silt. The waters of the China sea for one hundred miles or more out to sea are yellow at the surface with loess-mud along six hundred miles of coast; and when we consider the extreme slowness of deposition of this fine material, we cannot doubt that below the surface the silt extends eastward to the Kurosiwa, and follows the lower strata of this current far to the northeast.

This rapid transfer will, sooner or later, bring all the loess into the ocean, and thenceforth the waters of the streams and the coast will be clear, or alternately clear and turbid, in proportion to the absence or frequence of oscillations of the coast level.

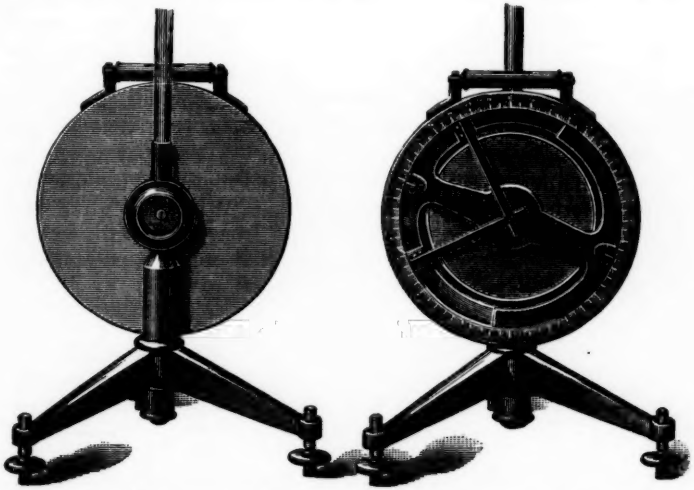
Environment and plasticity have been described as interregnant directing forces in evolution. It is during this period of turbid waters that the influence of environment and plasticity must be the most important factors in the evolution of life-forms; usurping, for a greater or less length of time, the position held for long ages by the purely biological factors—survival of the fittest, and heredity.

The very fine marine sediments are probably for the greater part the rapidly removed products of long-continued, protected disintegration and decomposition. If this be so, then the finer sand and clay rocks of any given column should record the extermination of all forms which, no matter how vigorous otherwise, were not able to adapt themselves to the radical change in their physical surroundings; and they should record also the operation of inorganic environment which then came in to break rudely the chain of descent, and introduce, during an interregnum, new laws of development under which grew up new forms.

In the removal of the residuary products of disintegration, glacial action and wind action perform parts that are nearly complementary to each other; for the removal by wind presupposes a central continental climate in which the dryness has temporarily become sufficiently extreme to destroy the vegetation, while a general glacial action presupposes a very moist climate.

ART. XVII.—*A method of Determining the Dip;* by N. D. C. HODGES.

THE magnetic polarity of a bar of soft iron is greatest when the bar lies in the line of the dip. This position of maximum intensity might be tested for by a small needle hung near one of the poles. This or similar ways have been used. The objections are that the rate of change in the polarity is small as the bar approaches the position sought, and the testing needle takes a less and less sensitive position, since the farther it is turned from the meridian the greater becomes the movement of the force of the earth upon it tending to bring it back. If, instead of one bar, two bars joined at right angles at a point near their ends be used, these difficulties may be avoided. When such a compound bar is placed so that the two branches make equal



angles with the line of dip, the effect on one of them is to make the lower end a north pole and its upper end a south, on the other the lower a south and upper a north. Supposing the bars exactly similar, the two poles at the point where they are joined, will neutralize one another; and any needle suspended near that point will hang in the meridian unaffected by the bars. A slight turning of the compound bar will render the field in which the needle hangs either a north or south, and it will move to correspond. To eliminate the effect of any permanent magnetism in the iron, four readings may be taken; first

both bars above the line of dip, then both below or in a position nearly 180° from the first, finally moving them so as to present the other side to the needle in each of the first two positions.

The first series of observations were made to see how constant the readings would be and how sensitive the method. They were all taken within twenty-four hours. After two of them the testing needle was changed from hanging horizontally to hanging at an angle of 45°. This was done to see if the poles at the outer ends of the bars had any influence. If the axis of the testing needle made equal angles with the two bars, the outer poles being at equal distances from the poles of the needle, would counteract each other as far as turning the needle. The results were constant within the limits of observation. The separate readings differed much more from the mean in the beginning. The bars seemed nearly free of permanent magnetism finally.

1st Series.

A,	77°·	77°2	76°7	75°6	74°5	73°5	73°1
B,	71·	70·7	70·6	71·6	72·7	73·8	74·2
C,	70·3	70·7	71·	71·8	73·	74·4	74·
D,	76·2	75·9	76·5	75·9	74·4	73·1	73·
Average,	73·6	73·6	73·7	73·7	73·7	73·7	73·6

The second series shows the effect of magnetizing one of the bars. The first three were taken in the morning, the last two in the afternoon, after the magnetizing. While the latter were being made, the magnetic state of the iron was rapidly changing, as the corresponding readings placed opposite one another show, and rendered any accuracy impossible.

2d Series.

76°2	70°5	69°9	87°5	84.5
77·	76·4	76·1	57·5	60·8
76·9	77·2	76·4	72·	70·
70·3	70·1	70·5	79·5	80·
73·6	73·6	73·2	74·1	73·2

The third series gives the results obtained on trying to remove the magnetism by rapping the bars with a hammer after each determination. The separate readings approached the value of the inclination. The averages remained constant within the limits of the instrument.

3d Series.

84°3	83°2	80°9	80°2	80°3
60·2	62·5	65·4	66·4	66·8
69·6	70·1	72·6	73·	72·4
81·	79·3	76·3	75·2	76·7
73·9	73·8	73·8	73·7	74·1?

The fourth series of results are those before and after heating the bars to redness for three or four hours.

4th Series.

Before heating.		After heating.				
66°4	66°2	67°7	68°4	68°5	68°9	68°9
79·9	80·5	77·5	78·9	78·3	77·7	77·5
76·3	76·	79·1	78·7	78·2	78·6	78·6
72·1	72·2	70·	70·	70·1	70·1	70·
73·7	73·7	73·6	74·	73·8	73·8	73·75

The instrument used was constructed to test the principle of the method. The accompanying cut shows quite clearly the different parts. The views are of the front and back of the divided circle. The iron bars are shown in the position to the north and above the line of dip, the initial position of the above description. They are on a frame which turns about the center of the circle, and from which they can be easily detached and reversed. The other plan shows the testing magnet in the casing at the middle of the disk.

No particular care was taken to have the plane of the circle in the plane of the magnetic meridian. As a self-registering apparatus, the instrument seemed faulty; as at no time during the experiment did the magnetic state of the iron remain constant.

Physical Laboratory, Harvard College, Dec. 6, 1878.

ART. XVIII. — *On a Group of dissimilar Eruptive Rocks in Campton, New Hampshire;* by GEORGE W. HAWES.

AMONG other results of the petrographical studies made by me under the direction of the New Hampshire State Survey,* I have shown that the rocks of the dikes, abundantly scattered through the White Mountains, are very diverse in composition and in mineral constituents. Independently of the results of decomposition, which has in many cases widened original differences, rocks which to the eye appear identical are often found, on microscopic examination, to be fundamentally different; and the rocks of closely adjoining dikes not infrequently have nothing in common save their geological position. This feature is quite striking, especially when considered in connection with the uniform character of the eruptive rocks in some adjoining regions. To illustrate it I have made a study of a small group of dikes in Campton where this diversity is very well exhibited.

The Livermore Falls are in Campton, but they are only two

* Geology of New Hampshire, Hitchcock; Part IV; Mineralogy and Lithology.

miles distant from the larger and more accessible town of Plymouth. The Pemigewassett river has here cut a gorge through a hill, and in the walls of this gorge the eruptive dikes are very conspicuous. The gorge is not long, and the dikes, five in number, are all embraced in a portion of it which is little more than a hundred yards in length.

Attention was called to these dikes in 1837, by Professor O. P. Hubbard of Dartmouth College.* His description of them is accompanied by a picture of the gorge, which shows their forms and relative position; but as the rocks could only be identified by microscopic examination, he did not attempt to classify them.

The rock through which the dikes intrude is mica schist, which presents its usual diversities, caused by variation in the proportion of the essential ingredients and the presence of accessories. The strike is northeast and the dip is variable. These rocks are considered to be as old as the Silurian, and Professor Hitchcock regards them as still older.

The five dikes cut the schist almost at right angles; all are nearly vertical, and parallel to one another. A bridge has been built across the gorge from which all the dikes can be seen except the one directly under the bridge. From their position with reference to the schists, it is inferred that the fractures resulted from the action of the same forces acting in the same way. Yet among these five dikes, there are found four very well distinguished rock species. I will describe these rocks in the order in which they occur, beginning with the one highest up the stream.

Dike No. 1, is seen only upon the left of the stream. It is about three feet wide; the rock is black in color, compact and apparently nearly homogeneous. The study of some thin sections indicates that it is a *diabase*. It was originally a mixture of augite, a triclinic feldspar and titanic iron, but all its ingredients are partially altered. The augite is in process of alteration into hornblende; some of its grains being still intact, some being partially and others wholly altered. The feldspar is more or less changed, but shows its polysynthetic character throughout. The titanic iron oxide is extensively altered into the grayish white product that is called leucoxene. Minute apatite crystals are seen in the section. Calcite, as a decomposition product, fills cavities that are apparently made by the removal of some other mineral; these cavities are also often partially filled with analcite, which has a cubic cleavage and exerts a very feeble action upon polarized light. The analyses of all the rocks described are placed together on a subsequent page.

Dike No. 2 is eight feet wide. The rock is black in color,

* Am. Jour. Sci., I, vol. xxxiv, p. 105.

and is composed of a very fine almost homogeneous ground-mass, in which small black shining crystals are porphyritically developed. The thin sections indicate that this is *diorite.* It is a mixture of hornblende, a triclinic feldspar, and titanic iron oxide. The large crystals are of hornblende; it is here an original product, and has not resulted from the alteration of pyroxene, as in the last case, since its well-formed crystals are developed in the common hornblendic forms. Many of them are twin crystals, the twinning plane being as usual parallel to the orthopinacoid. Both the large porphyritic crystals and the small ones in the ground-mass are fresh and unaltered. The feldspar is in part fresh and in part somewhat altered. The basic nature of this rock, as of the last, indicates that the feldspar is a variety low in silica, but its species can not be determined by optical means. The iron oxide is quite abundant, and in part well crystallized. Sometimes a large opaque hexagonal section is met with which is probably menaccanite. The rock contains a little apatite. Calcite, some zeolitic, mineral of an undetermined species and a little chlorite exist as decomposition products.

Dike No. 3 is ten feet wide, and is filled with a massive rock fine in texture and white or grayish in color. When a section cut from a white specimen is examined, the rock is seen to be composed largely of small, quite well defined orthoclase crystals. The meeting of these forms angular corners, that are for the most part filled with lime and iron carbonates and chlorite, though some are filled with quartz. In sections from the darker specimens it becomes evident that the aggregate in the angles is a decomposition product, for remnants of a dichroic green mineral, which is probably hornblende, are left there. It contains in addition some magnetite and some specimens show a little pyrite. The rock is a very fine grained syenite similar to those which occur in different parts of the State.

Dike No. 4 is about a hundred feet from No. 3 but is identical with it in all respects. Dike No. 3 separates into two branches in the middle of the stream, and forms two dikes in an island there situated, and it is not improbable that No. 4 may unite with it at some point.

Dike No. 5 is about seventy-five feet from No. 4. It is very narrow, being only about a foot wide, but it has several branches as wide as itself which unite with it at acute angles. This again like No. 2 is composed of a fine grained ground-mass in which larger crystals are developed, but when the sections are examined it is found to be an *olivine diabase.* The porphyritic crystals are in part perfectly formed augite crystals, and in part well formed olivine crystals which are mostly changed to serpentine. The finer portion of the rock is com-

posed of augite, a triclinic feldspar, titanic iron and minute brown dichroic crystals of hornblende. Some small amygdaloidal cavities were observed containing sphærosiderite, calcite and analcite.

In these five closely adjoining dikes there are, therefore, four very different kinds of rocks. Selecting specimens as fresh as possible from the different dikes I analyzed them with the following results:—

	Diabase. Dike No. 1.	Olivine diabase. Dike No. 5.	Diorite. Dike No. 2.	Syenite. Dikes Nos. 3 & 4.
Silica	41·63	42·77	41·94	58·25
Alumina	13·26	14·06	15 36	18·22
Iron sesquioxide	3·19	2·72	3·27	1·07
Iron protoxide	9·92	8·34	9·89	5·96
Manganese protoxide	·27	·15	·25	·10
Titanium dioxide	3·95	2·35	4·15	*tr.*
Lime	8·86	11·47	9·47	1·51
Magnesia	7·31	9·72	5·01	*tr.*
Soda	2·49	1·89	5·15	4·19
Potash	3·32	1·43	·19	5·59
Carbon dioxide	5·20	1·62	2·47	4·75
Water	1·35	2·74	3·29	·85
	100·75	99·26	100·44	100·49

Though I have mentioned the existence of decomposition products, they are present only in minute quantities; and as in these very compact rocks the new compounds must have been formed from the old, I think the above analyses represent very nearly the original composition of the rocks, with, however, the addition of the water and the carbon dioxide.

Between the light and dark colored rocks there is a wide difference, which indicates that the reservoirs from which they were ejected contained fused material of very different compositions. The black rocks are nearly alike in composition, but their differences are such as might account for the variation in mineral constituents. The quantivalent proportion of the sesquioxides to the protoxides is considerably higher in the diorite than in the diabases, and this is a condition favorable to the formation of diorites, as shown by the higher percentage of alumina usually found in the hornblende of eruptive rocks. The larger percentage of magnesia may have favored the formation of olivine in one diabase and not in the other. But the presence of compact and porphyritic materials in different dikes, though of nearly the same composition, indicates different conditions of cooling and crystallization, and these may also have been a cause of the mineral distinctions.

In the adjoining Connecticut Valley the red sandstones are cut by numerous dikes. Many of these rocks and others geo-

logically related have been microscopically examined by E. S. Dana,* and some were analyzed by myself.† It was shown that these large dikes which are so characteristic of the Mesozoic red sandstones of this coast are, wherever found, essentially uniform in composition and mineral constituents. They are compounds of labradorite, augite and magnetite, and vary only in the extent of their alteration. Professor Dana has concluded from this uniformity and their wide distribution over the Atlantic slope from Nova Scotia to North Carolina, that the dikes reach to profound depths.

It is most probable that the large and small dikes that are so common among the crystalline rocks of New Hampshire, occupy fissures which were made during the elevation of the mountains. In the process of elevation, variable conditions must have been introduced in the strata at different places and times, on account of the conversion of mechanical work into heat, as has been shown by Mallet and others, and this would have modified the depth at which fused materials would be found beneath the surface. If partial crystallization took place before eruption, as in the case of many modern volcanic rocks, very variable conditions might also have been introduced at different times for their solidification. The Mesozoic sandstones referred to do not occupy a position that indicates a great strain upon the earth's crust at the time of fracture, but are found in areas of gentle subsidence, and the uniformity in the dikes that characterize these regions, when compared with the diversity in the dikes of the mountain region of New Hampshire, is as striking as is the contrast in the geological features of these two areas of eruption. A sinking of the earth's crust might result in profound fractures which would reach to the homogeneous zone beneath the sedimentary formation. The crushing attendant upon elevation might fuse sedimentary deposits at various depths, and produce fissures that would be filled with the most diversified material.

ART. XIX.—*Notes on the Mesozoic Strata of Virginia;* by WM. M. FONTAINE.

[Continued from page 39.]

Hanover Area.—This, the northern portion of the Richmond belt, shows considerable differences from the Richmond coal field. The structure is not that of a basin, but it contains one or more broad anticlinals, the beds on the west side dipping northwest against the Azoic, and on the east passing with a northeast and east dip under the Tertiary. We have here also two series. The lower has the general character of the lower

* This Journ., III, viii, 390. † Ibid, III, ix, 185.

series of the Richmond Coal field. It has no workable coal, and only a few thin and impure layers, at most, an inch or two in thickness. Plants occur, some of the species found in the Richmond coal field being discovered here also, with other, somewhat later types. Taken as a whole, this part of the belt contains, as Rogers long ago showed, strata somewhat younger than those of the productive portion of the belt lying farther south. Much of the vegetable matter seems to have been drifted into its present position. I observed a very curious patch of coal, very locally deposited, which seems formed entirely of the epidermal tissue of leaves, which have been drifted together and packed closely in layers. These leaves seem to have been principally those of *Macrotæniopteris magnifolia*. The upper series also presents a general character like that of the upper series of the Richmond coal field, but some of the peculiar features found in that field are here much more exaggerated. The exposures are very poor, and it is very difficult to make accurate examinations. The greater portion of this series is composed of sandstones with subordinate beds of shale. The sandstones of the uppermost portions, especially those found in the northern extremity of the tract, are remarkable for the unsorted and undecomposed character of the material composing them. This material is a mixture of quartz, pinkish feldspar and dark mica, easily recognized as coming from certain granitoid gneisses, which are found in force along the southwestern side of the area. On compression, this material forms a rock closely like the original gneiss. On the northwestern edge of the area, along the North Anna River, this finer matter has associated with it large, partly rounded stones, coming from the same granitoid gneiss. On this stream the stones make up the greater part of the mass. They are imbedded in a matrix of the above mentioned finer material. The stones are often quite fresh, and sometimes of very large size. I saw one granite bowlder ten feet through, and not fully exposed. Many are two, three, and four feet in diameter. I am not able to say what connection, if any, these stones have with the bowlders of the interior belts. They were deposited before the cessation of the emission of igneous matter, as they are penetrated by several trap dykes. They form certainly the youngest beds of the series. The dip is very obscure, but appears to be to the northwest. When these beds are traced to the eastern side of the area, where they pass under the Tertiary, we find that the number and size of the stones greatly decreases. The beds now take in fragments of the shale, and other strata of the lower series, which feature is never seen on the western side. This of course should be the case, if the eroding force acted from the west and southwest. These upper-

most beds pass, as it appears, up into the strata which occupy the border belts, and which here overlap, as stated before, this area. I have not seen any similar beds of stones, on the west side of the Richmond Coal field, and do not think that they exist there, though my observations are not extensive enough to decide the question positively. The lower and middle portions of the series are much like the upper series of the Richmond Coal field. They contain some thin films of coal, and some plants, too poorly preserved to be of much value in determining age, though so far as their evidence is to be taken, the series is certainly younger than the Richmond coal-bearing strata. A large amount of drifted vegetation, principally trunks and limbs of coniferous trees, now changed to lignite, or silicified, is found especially in the upper beds. The amount heaped up at certain horizons, is sufficient to give the appearance of small, locally developed coal seams.

The Fredericksburg Belt.—In this, also, we may recognize two series, a lower, about 150 feet thick, and an upper, still thicker. Both vary so much in the composition and structure of the beds, that it is difficult to give any correct idea of them without going into details, which would require too much space. The lower series, however, is more constant in composition, and may be generally recognized by its physical features, which is not the case with the upper. It is composed mainly of a curious sort of sandstone (Rogers' feldspathic sandstone) which possesses very little coherence. This is due to the fact that it is mainly composed of grains of sand, usually fine, surrounded by a non-plastic white earth, resembling kaolin. This is a composition which indicates that the sorting action of water has not been brought into play. This is farther indicated by the structure of the mass, which is usually affected with false bedding, and often resembles a mass of agglutinated sand. The planes of the false bedding are very short, and shift rapidly. The strata lie nearly horizontal, inclining slightly to the southeast. Small stones are often scattered through the mass, and beds, nests, or pockets, of larger stones, occur at different horizons. These stones are sometimes four to six inches in diameter. All the stones are either quartz, which is most abundant, or Potsdam quartzite. The lower portion is, on the whole, more siliceous and coarser. The upper part is often a fine-grained powdery mixture, of siliceous matter and kaolin, which shows very sudden changes of texture and a complicated cross-bedding. This material is very significant, as there is no rock in the Azoic of the adjacent country which could by its erosion yield it. The Azoic rocks to the west and northwest are mica slates, argillites, etc., which in decomposing yield yellow, reddish, and bluish clays, and yellowish or

reddish sand. From the character of the finer matter, and of its contained fragments, it is clear that much of it was derived from the Potsdam strata, on the west of the Blue Ridge. As I shall have frequent occasion to refer to this Potsdam material, a brief description of the occurrence and character of the parent rocks will be required.

In my article on the "Primordial Strata of Virginia," published in the May and June numbers of this Journal for 1875, I gave some account of certain varieties of the Potsdam strata, which occur in great force near Rockfish Gap, on the head waters of South River, a branch of the Shenandoah River, and at the entrance of the gorge cut by James River, at Balcony Falls, where it crosses the Blue Ridge. They also are exposed along the entire lower course of the Shenandoah, and on the Potomac, a short distance above Harper's Ferry. At this latter place also, they are much crushed and fractured. The rocks, at these points, are much shattered, and lie in their original beds so loosely, that they may be removed with a pick. The most notable of these rocks are compact, vitreous quartzites and peculiar sandstones, which have the grains of sand imbedded in a white, non-plastic, argillaceous matter, like kaolin. The latter graduate, by loss of quartz grains, into shales or slates of a white color, composed of the non-plastic matter. All are strikingly white and void of ferruginous matter. It is from these strata that the Potsdam material comes, which is scattered so abundantly over portions of the eastern border of the Atlantic slope, and this appears to be the source of the precisely similar white matter, which forms the finer material, of the lower series of the Fredericksburg Belt. As nothing like it is found among the Azoic strata, fragments of this material may easily be recognized when found east of the Blue Ridge. Indeed, they are as well characterized as fossils. Were there any doubt attending the assigning the source of these stones, from physical evidence alone, to the Potsdam, it would be put at rest by finding, as we often do, the well-known marks of *Scolithus linearis* in these erratics. This lower series is best exposed at the town of Fredericksburg, where it, in its more coherent portions, yields a building stone, which was formerly used to a considerable extent.

The lower series passes up into a higher system of beds, constituting the upper series, which is marked by a smaller proportion of the white incoherent beds, so characteristic of the lower, and by a predominance of clays of reddish, yellowish, and bluish colors, and of reddish and yellowish sands. These clays and sands increase in amount as we follow the belt northward. Near Alexandria, between Washington and Baltimore, and near the latter city, they constitute the whole of the upper

series. The material of these beds comes from the decay of the Azoic on the west. These clays and sands also are very irregularly bedded. The sands especially, are much affected by cross bedding. From Alexandria northward the lower series is rarely seen, being too deeply buried. At Baltimore it appears in the lowest white clays and sands dug in the base of the hills. Toward the close of the formation of these strata they were invaded by some agent which planed them off, or scored them more or less deeply into channels, and brought along with it quantities of sand, gravel, large rounded stones and slightly worn blocks, often of very large size. This material was deposited in a confused manner, and is usually very different from the beds on which it reposes. This agent seems to have acted from the northwest. From Fredericksburg to the Potomac, the gravel and rounded stones are composed of quartz, Potsdam quartzite, and sandstone. In the vicinity of Alexandria, Potsdam bowlders predominate over the quartz. They are commonly six or eight inches in diameter, and often attain the dimensions of one and two feet, showing frequently *Scolithus* markings. They are found over the surface of the Mesozoic here, five or six miles from the Potomac and up to the Azoic border, often forming belts of stones packed in comminuted Potsdam matter. Throughout the entire belt, the large, slightly worn blocks, are composed of crystalline rocks, from varying distances to the west and northwest, all the way to the Blue Ridge. These are not rarely four or five feet in diameter, and sometimes six or seven feet. Near Fredericksburg, we may see large stones formed of the characteristic epidotic and chloritic schists, composing the Blue Ridge on the west. Near Alexandria, large, slightly worn masses, having a diameter of four or five feet, are found of the peculiar siliceous and chloritic schists composing the Blue Ridge at Harper's Ferry, along with gneiss and mica schist from nearer localities. North of the Potomac, over the lignitic and iron-ore clays, between Washington and Baltimore, which belong to the upper series, I did not see any Potsdam material. The rounded stones and gravel are quartz, but the Azoic erratics abound. The iron-ore clays near the Relay House have been deeply scored, and the excavations filled with coarse sand, gravel, masses of the iron-ore, and Azoic erratics. But this action does not seem to have been confined, in all places, to the upper portion of the series. Near Neabsco Station, on the Fredericksburg and Alexandria Railroad, I found near the Azoic floor, and under a very considerable thickness of the upper sands and clays, a heterogeneous mass of angular Azoic matter in large blocks mixed with worn and rounded stones, of quartz and Potsdam sandstone, imbedded in blue clay. This

drift matter, by the eroding and sorting action of the streams which cross it and pass into the Tertiary or Cretaceous areas, has been carried over to a distance, greater or less according to the particular conditions, on the surface of the more recent strata. It has by the same action been concentrated near the streams. Also in the space between these rivers, surface erosion and other causes have caused it to pass over a short distance on the newer beds, but it soon disappears within the areas occupied by such beds. In Virginia we have no means of determining whether the drift follows the surface of the upper series as it passes eastward under the Tertiary, but in Maryland it evidently passes eastward under the Cretaceous. Thus the wells bored to the southeast of Baltimore penetrate it on entering the clays and sands of the upper series. I have carefully examined the Maryland Reports for information on this head, and find that they give evidence that this bowlder deposit passes as far to the southeast as its horizon has been reached.

Fossil Plants.—These can be only briefly noticed. I have found a large number of well preserved plants at Fredericksburg in the lower series. They occur in a gray clay or shale, which rests on a bed of cobble-stones. I owe the discovery of the deposit to Professor P. R. Uhler, President of the Maryland Academy of Sciences. The material which imbeds the plants is not laminated, and the plants all seem to have been drifted. Considering their mode of occurrence, the small amount of change exhibited by some of them is wonderful. As the deposit is very local, it has hitherto escaped attention almost entirely. Many of the plants retain their entire structure, and are only colored brown. I have some which seem still to possess their chlorophyll! They have a decidedly green color. They are principally Conifers, Cycads and Ferns. No species of the Richmond Coal field are found here, but a few show affinities with Rhaetic plants. With the plants above named, I find certain netted-veined leaves, which by their nervation cannot be distinguished from Angiosperms. Had they been found with Cretaceous or Tertiary plants I think no one would hesitate to consider them as such. As, however, they occur with a well marked Upper Jurassic flora, I hesitate to pronounce them to be Angiospermous plants, without a more careful study and extended comparison, than I have as yet been able to make. They are certainly not "Dictyophyllum," which is the genus of fossil ferns that stands nearest to them. But when we find such a development of undoubted Angiosperms in the lowest Cretaceous beds of New Jersey and of the west, we should expect to find, at least, their ancestors in the Jurassic flora.

This flora, as a whole, is decidedly younger than that of the Richmond Coal field. Its rather complex type will require a

careful study to fix the age, but from my preliminary examination, I conclude that it is nearly of the age of the Upper Oolite of England, an age long ago assigned by Rogers to these beds. The general facies is much like that of the flora of Sutherland in Scotland, figured in part by Hugh Miller in his "Testimony of the Rocks," which he considered as being partly Liassic, but which Mr. Judd shows to be all Upper Oolite in age. R. C. Taylor found at another locality near Fredericksburg, but on the same horizon with mine, a few plants which he figured and described, in a communication published in the Trans. Geol. Soc. Penn., vol. i, 1835. These plants he considered to be Oolite in age.

In the upper series at Fredericksburg I have found, in a state of preservation permitting determination, only a few twigs of Conifers with leaves. But this series, both here and to the northward, is remarkable for the great quantity of lignite and jet which it contains, produced from the trunks and limbs of coniferous trees. The most abundant tree seems to have had a wood very like that of the white pine (*Pinus strobus*). To judge from the amount of this lignite in the upper series of the belt, the country must have been covered with extensive forests of coniferous trees, at the time of the formation of the beds composing it. The trunks are sometimes piled so thickly over each other that they produce the appearance of a prostrate forest. Near Neabsco Station we find such a state of things. The trunks are here flattened and imbedded in a blue clay. Great quantities of lignite occur in the clays of the upper series, especially the clays which Mr. Tyson calls "iron-ore clays," which are found between Washington and Baltimore. In these clays, stumps of Cycads, belonging to at least two new species of the genus *Cycadoidea*, are found. The blue clays at Baltimore yield some beautiful ferns of Wealden type. All the attainable evidence points strongly to the conclusion that the age of these upper beds is Wealden. Professor Rogers mentions that they yield at Fredericksburg two or three species of ferns and stumps of Cycads of the genus *Cycadoidea*. None of these have been seen by me. What the relation of these beds is to the lower clays of New Jersey with Angiospermous leaves, is not known, though, no doubt, the New Jersey beds are somewhat younger.

Professor Wm. B. Rogers has long held the opinion that these upper beds are Wealden in age. This is also the opinion of Professor P. R. Uhler. Mr. P. T. Tyson also held a similar belief which he based not only on the plants, but on certain fresh-water shells (Unios?) which resemble Wealden species. The beds in question, however, seem to be by all others considered as Lower Cretaceous, as it would appear, solely from their underlying known Cretaceous marine strata.

[To be continued.]

ART. XXI.—*Notices of Recent American Earthquakes.* No. 8; by Professor C. G. ROCKWOOD, Jr., College of New Jersey.

IN these notices, as heretofore, those based upon *single* newspaper notices and which could not be otherwise verified, are printed in smaller type and the source of the information is indicated.

For information received, I am again indebted to J. M. Batchelder, Esq., of Boston, who has kindly given me the benefit of his lists; and to the U. S. Monthly Weather Review.

1877.—July 13. From Coban, a place in the central part of Gautemala, east of the mountains, there were reported thirteen or fourteen shocks at 5.15 A. M., the direction E. to W.—(U. S. Weather Rev.)

From the same place are the following four.

July 20. At 10.05 A. M. two slight shocks, E. to W.

July 27. At 8 P. M. a slight shock, lasting a few seconds.

Aug. 27. At 11.35 A. M. three shocks from the north.

Sept. 10. At 10.45 A. M. two shocks, lasting seven seconds and sufficiently strong to sway the façade of the church.

Sept. 10. A slight shock felt about 2 A. M. at Cambridge, Mass. Eight hours later the shock occurred along the Delaware River, already noted, III, xv, p. 25.

Nov. 16. The shock about 2.30 A. M. at Knoxville, Tenn., as noticed, III, xv, p. 27, was from S.W. to N.; and was felt also at Murphy, N. C., where the direction was W. to E. and the duration fifteen seconds.

Nov. 21. At Coban, Gautemala, at 10.16 A. M. two vertical shocks, and at 10.37 P. M. a number of small shocks lasting forty seconds.

Nov. 24. At Red Bluff, Cal., two shocks at 6.30 and 6.50 A. M., the first lasting twenty seconds, and being from E. to W. This was felt also at San Francisco.

Nov. 26. At Coban, Gautemala, at 9.57 A. M. a few vertical shocks lasting twenty seconds.

Dec. 14. A strong shock at 7.15 P. M. in Callao, Peru, the undulation being from N. to S.—(N. Y. Times.)

Dec. 18. At Beachburg, Ont., two shocks, the first between 1 and 2 A. M., the second between 5 and 6 A. M. and quite severe.

1878.—Jan. 2. A slight shock about 7 P. M. in Louisa and Hanover Counties, Va., accompanied by a roaring sound.

Jan. 8. Two slight shocks at 10.30 P. M. at Cairo, Ill.

Jan. 23. At 7.55 P. M. a strong shock, lasting thirty seconds, occurred on the southern coast of Peru. It was severe at Iquique and Arica, and was accompanied by a subterranean

sound; but it was most destructive in the interior, where some houses were shaken down. But little damage was done on the coast; the sea was comparatively quiet and there was no tidal wave.

At Iquique lighter shocks followed with short intervals during the 24th and 25th, over *forty* distinct shocks being counted. A second heavy shock occurred at 8.30 P. M. of the 24th.

Jan. 27. The harbor of Callao was greatly disturbed by a series of tidal waves. The heavy surf began about 3.30 A. M. and continued during the day, causing great damage to the docks and sea-wall, inundating the English railroad station and resulting in some loss of life. The movement came from the north.

Feb. 5. A shock at 11.20 A. M. at Flushing, N. Y., sufficient to break windows.—(U. S. Weather Rev.)

Feb. 26.—A shock at 11.56 A. M. at San Francisco consisting of three vibrations N. to S., lasting five seconds.—(U. S. Weather Rev.)

Feb. 27. A severe earthquake occurred at 5 P. M. at Reykjavik and other places in the southwest part of Iceland. It was connected with a volcanic eruption which began the same night and continued for more than a month. The new volcanic openings, fourteen in number, were situated in the Raudaskal valley, about four miles northeast of Mt. Hekla.

March 12. At 4 A. M. a severe shock at Columbus, Ky., causing the fall of a portion of the bank of the Mississippi.

———. The same day two shocks at Milford, Vt.—(U. S. Weather Rev.)

March 17. Two sharp shocks at St. Thomas, Lower California.—(U. S. Weather Rev.)

March 18. A slight shock at 6.30 A. M. at Tacoma, Washington Ter.—(U. S. Weather Rev.)

April —. "In the early part of the month," an earthquake occurred at Manizales, United States of Columbia, overthrowing a church tower.

April 12. About 8.40 P. M. a destructive earthquake occurred at Cua in Venezuela. This was a town of about 3,000 inhabitants, situated on the River Tuy, in N. lat. 10° 8′ 15″ and W. long. 66° 55′ from Greenwich, the center of a flourishing agricultural district. The height above the Caribbean Sea is 232 meters. The center of the town is situated on a small hill about 20 meters above the lower part. The hill is composed of gneiss, micaceous and chloritic schists, rising rather steep toward the W.S.W. It is surrounded by strata of clay and marl, covered by a deep stratum of alluvial soil, and resting on dark limestone and argillaceous schists. This upper town was

destroyed, being reduced to a heap of ruins in less than two seconds. About 300 lives were lost and property destroyed to the amount of £300,000. The lower town suffered very little. The direction of the shock was from E. 15° N. and the angle of emergence was found by A. Ernst of Caracas to be about 60°. The center could not have been very deep, as the destruction was limited to an area of one square mile. The transverse wave however was felt one hundred miles distant and occurred in Caracas, distant in a straight line twenty-six English miles, at $8^h\ 41^m\ 34^s$. Lighter shocks continued to be felt at intervals up to May 4.

For the greater part of the above I am indebted to an article by Mr. Ernst, in Nature, vol. xviii, p. 130.

April 15. At Glendive, Montana, on the Yellowstone River, there were three shocks, following each other at intervals of half an hour.

April 23. At Loreto, on the Gulf of California, a severe shock of two or three seconds at 10 A. M.; the first of a series lasting till May 3.—(U. S. Weather Rev.)

April 28. A severe shock, felt at Caracas at 8.30 P. M. destroyed a large part of the town of Ocumare, about twenty miles east of Cua and in the same valley.

April 29. Shocks were again felt at LaGuayra, Caracas, Porto Cabello and Valencia, in Venezuela.

May 8. At 8.25 P. M., a shock from N. to S. and sufficiently violent to stop clocks, was felt in the valley of the Sacramento Cal., from Red Bluff to Sacramento City, and also west of the Coast Range in Mendocino County.

May 14. A severe shock was felt at Guayaquil about 6.40 P. M., preceded by a loud noise. There was a less violent shock at 9 A. M. of the 15th.

May 15. A severe earthquake occurred at 8.35 P. M. at Tacna and Arica, Peru.

May 21. A shock at San Bernardino, Cal.—(U. S. Weather Rev.)

June 4. A light shock at 12.28 P. M. at San José, Costa Rica.

June 9. A strong shock at the same place at 4.34 P. M.

June 11. On the night of the 11th and 12th, four shocks were felt at Los Angeles, Cal., as follows; at 11.12 P. M. a distinct shock, at 11.20 P. M. a violent shock, duration five seconds, motion N.W. to S.E.; at 2.30 A. M. a light shock; and at 6.30 A. M. a slight tremble.—(U. S. Weather Rev.)

June 14. A slight shock at Cimarron, N. M.—(U. S. Weather Rev.)

July 2. Two light shocks at Campo, Cal., at $5^h\ 55^m\ 30^s$ (A. M. or P. M. ?) from S.E. to N.W. with a noise resembling thunder.—(U. S. Weather Rev.)

July 11. A severe shock at midnight at St. Thomas, W. I.
July 16 and 18. Two severe shocks in Hayti.

July 21. A slight shock at 5 A. M. at Salt Lake City.—(U. S. Weather Rev.)

July 26. A slight shock, direction north and south, was felt at 8.25 A. M. at Los Angeles, Cal., and at San Gorgonio, San Bernardino and other places in the mountains east of Los Angeles.

July 27. At 7.30 P. M. a strong shock at San José, Costa Rica.

Aug. 3. A severe earthquake occurred at 2.15 P. M. in the Island of Martinique, W. I. The vibrations continued fifteen seconds, and in the town of Diamant several buildings were thrown down.

Aug. 9, 13, 14, 15, 22, 30. At San José, Costa Rica; on 9th at 4.15 A. M. a feeble shock; on 13th at 7.17 P. M. a strong shock, at 11.30 P. M. a feeble shock; on 14th at 3 A. M. and 12.48 P. M. feeble shocks; on 22d at 10 P. M. a strong shock; on 30th at 0.23 A. M. a feeble shock. At Cartago, twelve miles east of San José, five shocks occurred from 7 P. M of the 14th to 5 A. M. of the 15th.

Aug. 29. Letters from Alaska, dated Sept. 1, state that frequent shocks had occurred during the summer, in connection with renewed activity of the volcanoes of the Aleutian Islands. On Aug. 29, the village of Makuslin on Unalaska Island was destroyed by earthquake shocks and tidal waves.

Sept. 7. Three shocks from west to east at San Francisco about 9.35 A. M.—(U. S. Weather Rev.)

Sept. 24, 29. At San José, Costa Rica, feeble shocks at 5 A. M. and 6.55 P. M. of 24th, and at 7.45 A. M. and 7.15 P. M. of 29th.

Sept. 29th A shock about 6 P. M. at San Francisco and Oakland, Cal., from N.E. to S.W.

Oct. 2. At 6 P. M. a severe earthquake occurred in the southern part of San Salvador, Central America. The village of Jacuapa was nearly destroyed and much damage was done in neighboring places. The movement lasted over forty seconds and was from southwest to northeast. Eruptions were feared from the neighboring volcanoes, and advices a few days later report great activity in the volcanoes Izalco and Santa Ana.

———. The same day a slight shock was felt in Santiago de Cuba.—(U. S. Weather Rev.)

Oct. 4. At 2.30 A. M. a shock was felt along the Hudson River from Marlborough to Peekskill, N. Y., a distance of about twenty-five miles. It was sufficiently violent to awaken

persons and at some places was accompanied by a rumbling noise. The direction was north to south.

Oct. 9. A severe earthquake at Manizales, the capital of Antioquia, United States of Columbia, destroyed over a hundred houses, including the church, hospital and city buildings.

Oct. 11. At San José, Costa Rica, a feeble shock at 5 P. M.

———. The same day at 7.30 P. M. a severe shock at San José, Cal., accompanied by a rumbling noise. The vibrations were north and south and lasted thirty seconds.—(U. S. Weather Rev.)

Oct. 21. At 5.40 P. M. two shocks from north to south at Sacramento, Cal.—(U. S. Weather Rev.)

Nov. 11. At 9.45 A. M. a slight shock from east to west at San Francisco.—(U. S. Weather Rev.)

Nov. 18. A shock was felt at St Louis, Cairo, Memphis, Little Rock and other places in the Mississippi Valley. The direction was generally north to south. The time was reported by the U. S. Signal Service observer at Cairo as $11^h\ 51^m\ 50^s$ P. M. He says, "a trembling was felt, lasting forty seconds, followed by a rocking motion from W.N.W. to E.S.E. lasting twenty seconds, and a second trembling lasting ten seconds." Another slight shock was felt at Cairo at 5.10 A. M. of the 19th, the direction of which was also W.N.W. to E.S.E.

Nov. 23. At Murphy, N. C., a slight shock at 10 A. M. from west to east, with a rumbling noise.—(U. S. Weather Rev.)

Princeton, N. J., Jan. 1, 1879.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Note on J. C. Draper's paper "On the presence of Dark Lines in the Solar Spectrum which correspond closely to the lines of the Spectrum of Oxygen."*—The paper above referred to appeared in the October number of this Journal. A cursory glance at it gives the impression that the methods had been carefully criticised beforehand, that the experiments had been made with minute accuracy and that the results were trustworthy; but closer examination of it raises most serious questions on all the points mentioned. Errors of method and of experiment appear which make it quite impossible to accept the conclusions reached. It is the purpose of this note to point out some of these.

In the first place the author throughout the paper confounds Ångström's scale numbers with wave lengths. Thus, for example, p. 257, he says, line 18, the photographs were "in sections of eighty to one hundred wave lengths," line 24, "each wave length being five millimeters in extent," and line 34, "each section of one hundred or more wave-lengths;" p.

258, line 24, "Error amounting to half a wave length could therefore exist in the position of a line, according as it fell on one side or the other of a figure on the scale expressing a wave length;" p. 259, line 18, one iron line "to every eleven wave lengths was used;" p. 261, line 18, no element gives "a line within two or three-tenths of a wave length of that position;" p. 264, line 16, "no other element furnishes a line which falls on the same wave length." In proof that he really means scale divisions, he gives a section of his chart on p. 259, and says, line 38, "On the first space below the line is the scale of wave lengths, each wave length being five millimeters in extent;" and p. 260, line 28, "in the eighteen wave lengths represented in the diagram." There are eighteen scale divisions in the diagram, each scale division being five millimeters long. Again, p. 264, referring to the coincidence of oxygen and solar lines in his table, he says, line 2, "in four, the difference is only five one-hundredths of a wave length; in twenty-two, ten one-hundredths of a wave length; in four, fifteen one-hundredths of a wave length; in eleven, twenty-one one-hundredths of a wave-length; and in the remainder, the greatest difference is only thirty-five one-hundredths of a wave length." Referring to the table, the four lines first mentioned are given as 3982·75, 4075·50, 4345·15 and 4483·80; the corresponding oxygen lines being given as 3982·70, 4075·45, 4345·20, and 4483·75. The difference is obviously five one hundredths of a scale division, not of a wave length. From the fact that this error runs through the entire paper, it would almost seem as if the author was not aware of the distinction between wave lengths and scale numbers. Using Ångström's scale he confuses wave lengths with ten millionths of a millimeter; whereas in the case of D for example, the wave length is nearly 6,000 times greater. If the author really means what he says, he asserts that the wave length of the mean ray of the spectrum is one two hundred and fifty-millionth of an inch instead of about one forty-thousandth, as we know it is.

Second, the author deems it of the greatest importance in the preparation of his solar photographs to use reflected rays exclusively; saying, p. 256, last line, "*at no time did the solar rays pass through glass;* all error that might arise during refraction was thus avoided." After this virtual condemnation of the use of refraction at all, he not only uses for comparison Ångström's wave lengths made with achromatic lenses and a refracting grating, constructing even his chart upon them as a basis, p. 258, line 7, "the values assigned to the wave lengths in this chart are those of Ångström;" but the very spectrum of oxygen by which the coincidences of the lines of this element with those of the sun spectrum were to be established, was photographed with glass prisms and achromatic lenses.

Third, the author states that the prisms with which the spectrum of oxygen was photographed were adjusted "to the minimum deviation of D′." Supposing D_1 to be meant, this

precaution, which gives the appearance of extraordinary accuracy to the adjustment, is practically an impossibility with the apparatus employed. Minimum deviation of the D line as a whole could not under these circumstances be distinguished from that of either of its components, nor could that of D_1 be distinguished from that of D_2. Moreover, it is difficult to understand why he adjusts to minimum deviation for D′ and not for G, near which the work is to be done. Instead of D′, the line for which his apparatus was adjusted should have been chosen in the photographic portion of the spectrum, for example between G and H.

Fourth, on page 265, line 25, the author says that this "is a problem not to be solved by the comparison of two spectra of small dispersion." Hence it is a matter of some surprise to find that in getting his oxygen spectrum, he uses only "two flint glass prisms of 60°" and for objectives, "achromatics of ten inches focus." The bright line spectrum of oxygen taken by Henry Draper, which the author in this paper inferentially attacks, was made, as we find on examination, with a direct vision battery of nine prisms and an observing telescope of forty-two inches focal length. The original negatives taken with the latter apparatus, must consequently have been eight or nine times as long as the author's; and even these were none too large for the proper solution of the question.

Fifth, the author seems to have attempted to compare together a diffraction spectrum of the sun with a prismatic spectrum of oxygen. Such a comparison, by the method adopted is manifestly of no value. Owing to the irrationality of dispersion of various refractive media it is an extremely difficult thing to compare accurately two prismatic spectra of different kinds. But the matter rises to an absurdity when a comparison is attempted between a grating spectrum and a prism spectrum. The graphic method, employed to supplement the direct method, does not appear to help the comparison, since the author nowhere gives both coördinates to the curve constructed.

Sixth, it is more than questionable whether the measurements of the solar spectrum lines actually made by the author are capable of the accuracy he assigns to them. The values in his table of wave-lengths are given to one hundredth of a division of Ångström's scale. As the author says on page 257 that each division of this scale, which is one millimeter, was enlarged to five millimeters upon the paper scale on which the photographs were projected, to measure to one hundredth of a scale-division would require the measurements on the screen to be made to one-twentieth of a millimeter or the one five-hundredth of an inch, about; a degree of refinement highly improbable under these conditions. Moreover the accuracy of the results of such measurements is seriously impaired by the variation in the position of the lines on the screen, due to the fact that the large number of negatives (eight or nine apparently), required to give the whole photographic spectrum, must, unless special precaution was taken (of which

there is no evidence), have been made with glass of different thicknesses. When projected in the lantern, this variation in thickness would necessitate a change in focus and so cause a change in the magnifying power. The smaller sizes of photographic glass vary in thickness from one to two millimeters. Consequently the displacement of the lines due to the difference of magnifying power arising from this cause would exceed considerably the limit of measurement, which, as above stated, was the one five-hundredth of an inch. But another and a more serious cause of inaccuracy must here be pointed out. From the data given by the author, it may readily be calculated that his original photographs of the oxygen spectrum, taken with two prisms of 60° and with lenses of ten inches focus, could not have been over half an inch long in the region from G to H. Since Ångström's chart from G to H is sixteen inches long, the author's spectrum would have to be magnified thirty-two times to make it the size of this. But as each millimeter of Ångström's scale was made five millimeters on the author's scale of measurement, the original negative as thrown on the screen must have been magnified one hundred and sixty diameters. Any one who has worked at all in spectrum photography, knows that it is utterly futile for purposes of measurement, to magnify a photograph taken under these circumstances, as much as this, since then the size of the silver grains becomes larger than the details of the picture. In the absence of any precise statement the reader has to make the calculation for himself; but the figures above given cannot be far astray.

Seventh, there is only an appearance of accuracy when the attempt is made to fix the position of the oxygen spectrum lines to hundredths of one of Ångström's scale divisions. The projection method by which the solar lines were measured, has already been proved inadequate. And as to the method of graphical interpolation, used as auxiliary to the lantern, it does not appear that as used by the author, it was capable of any such accuracy as that claimed. In constructing the curve, the iron lines are taken with Ångström's values for the wave-lengths; but these, though estimated to tenths were read only to whole divisions of the scale. Moreover, only forty-seven iron lines were used in all, or one to every eleven scale divisions; the reading being to one one-hundredth of a scale division, or 1,100 numbers to one iron line. Since the author measured no wave-lengths directly, he was obliged to construct a considerable "portion of the curve from the wave-lengths of oxygen and air lines already given by various authorities." These values were taken, page 258, from Watts' Index of Spectra. On referring to this book, the values are given only to the units place. And even then, discrepancies amounting to from three to five entire units, or from three hundred to five hundred times the author's limit, appear in the wave-length as given by the various authors relied on for the measurements employed in the paper before us.

Eighth, the author nowhere states the peculiar character of the lines in the oxygen spectrum and appears not to know that they have any. He has apparently taken it for granted that the lines of oxygen are intrinsically as sharp as the lines of the solar spectrum. But this, at least in many instances, is known not to be the case. Consequently it is quite impossible to measure the oxygen lines as accurately as the solar lines, and even these as has been shown cannot be measured to the accuracy which the author claims. Ångström himself admits that there may be an error of one-tenth of a division in his scale numbers.

It would seem sufficiently obvious from what has been said that the results given in this paper are entirely vitiated by the errors of method and of experiment which it contains. The author must not be confounded, because of the similarity of initials, with the distinguished investigator, Dr. J. W. Draper. G. F. B.

2. *On the Influence of Pressure on Chemical Action.*—Berthelot calls attention to the results of Pictet's experiments on the liquefaction of oxygen and hydrogen as illustrating his thermochemical views. The decomposition of potassium chlorate into oxygen and potassium chloride, as he has proved, is an exothermic reaction not limited by its inverse; hence it is not arrested by a pressure of 320 atmospheres. In fact the reaction $KClO_3 = KCl + O_3$, according to Berthelot's measures, evolves at the ordinary temperature 11 calories. About 400°, the chlorate being melted and the chloride solid, this quantity can only be increased. The same is true of the action of potassium hydrate upon the formate, the reaction by which Pictet prepared his hydrogen; the gas continuing to be evolved even under a pressure of over 600 atmospheres. This reaction is also exothermic and not limited by its inverse; the transformation represented by the equation

$$KCHO_2 + KOH = K_2CO_3 + H_2$$

evolving at the ordinary temperature 18·4 calories. At 400° or 500° all the bodies being supposed melted, the heat evolved would not be altered; because the heats of fusion of known salts do not exceed four calories and the initial system has two melted molecules, the final system has only one. Exothermic reactions then continue whatever be the pressure; but it is probable that their velocity is changed and perhaps the temperature at which they take place.—*Ann. Chim. Phys.*, V, xv, 149, Oct., 1878. G. F. B.

3. *On the Specific Heat and the Heat of Fusion of Gallium.*—Berthelot has determined the specific heat and the heat of fusion of gallium from an ingot weighing 34 grams, placed in his hands by Lecoq de Boisbaudran. The specific heat of this metal was determined in the author's water calorimeter, by the usual methods. Two experiments, made, the one between 119° and 30° the other between 106° and 12·5°, gave as the specific heat of liquid gallium (the melting point being 30°, but the state of surfusion continuing to near zero) 0·0802. The specific heat of solid gallium between 23° and 12°, was found to be 0·079. If the latter measurements be made too near the fusing point, the value obtained is too high.

The heat of fusion of gallium was determined by introducing a crystal into the surfused metal. The whole crystallized at once, and evolved heat which was measured. At 13°, the mean value for a unit of weight was 19·11 gram-degrees, the number remaining sensibly the same between 30° and 0°, because of the close agreement of the two specific heats. Referred to the atomic weight, 69·9, the heat of fusion is 1·33 kilogram-degrees or calories. The close correspondence of the two specific heats of gallium taken at nearly the same temperature, is true also of mercury, lead, tin and bismuth. The atomic heat of gallium in the liquid state is $69{\cdot}9 \times 0{\cdot}0802 = 5{\cdot}59$; in the solid $69{\cdot}9 \times 0{\cdot}079 = 5{\cdot}52$.—*Ann. Chim. Phys.*, V, xv, 242, Oct., 1878. G. F. B.

4. *On the Occurrence of Ytterbia in Sipylite.*—The mineral called sipylite was described by MALLET as a new niobate, as occurring in Amherst county, Virginia, associated with allanite.* An analysis was made of it by Brown in his laboratory, who found that, among other things, it contained 28 per cent of earths supposed to be erbia and yttria in the ratio of 27 to 1. At the request of DELAFONTAINE, a portion of this mixture of earths was sent to him by Mallet. Its pale yellow color indicated to him the presence of terbia or philippia, if not of both. But the feebleness of its absorption spectrum, and the very weak rose color of its nitrate and oxalate led him to believe that if no error had been made in determining the atomic weight, a new metal was present. The quantity of material being too small to decide the question, he succeeded in separating some sipylite from some allanite in his possession. From this a pale yellow earth was obtained, the nitrate of which showed in the spectroscope small quantities of erbia and philippia. But the high atomic weight 127 or 128, taken together with the weakness of its spectrum, indicated that the earth was new. By several partial decompositions of the nitrate, a feebly colored base was obtained, having an atomic weight near 134, giving colorless salts, yielding a sulphate like that of yttria, and crystallizing easily, a formate crystallizing in mamillated masses, and a double sulphate with potassium easily soluble in a concentrated solution of potassium sulphate. While he was looking for a name for the new earth, the description of Marignac's ytterbia appeared, the characteristics of which accord so well with that of his new earth as to leave no doubt of their identity. The discrepancy in atomic weight will be settled by new determinations.—*C. R.*, lxxxvii, 933, Dec., 1878. G. F. B.

5. *Upon the development of Electricity as the equivalent of chemical processes.*—In this paper H. F. BRAUN discusses the equivalence of heat and work, and electricity developed by heat and work. Some of his conclusions bear directly upon the question of dynamo-electric machines. He proves that with electricity of high tension, the per cent of potential electric energy which is converted into work is less, the greater, under like conditions, the tension is: That heat never can be wholly converted into electricity;

* This Journal, III, xiv, 397, Nov., 1877.

but only to the same degree to which it can be also converted into work.

If placing the three quantities in this order: (1) Electrical potential energy; (2) Mechanical work; (3) Heat. We have: 1 can be entirely transformed into 2 and into 3; 2 can be entirely changed into 3 but only partly into 1; 3 in general, can never be entirely transformed into 2 or into 1. The remainder of the paper contains the application of the foregoing laws to the theory of the Voltaic cell.— *Wiedeman's Ann. der Physik und Chemie*, 1878, No. 10, p. 182. J. T.

II. Geology and Mineralogy.

1. *The Lœss of Minnesota;* by N. H. Winchell. The following facts are taken from the Sixth Annual Report (for the year 1877) of the Geological and Natural History Survey of Minnesota, under Professor N. H. Winchell.—That the lœss deposit is the result of widespread diffusion of fresh water, at the time of the last Glacial epoch over those surfaces either drift-covered or not, which were not at the time affected by the glacier movement, is highly probable; but what the peculiar circumstances and causes of such gentle diffusion of nearly tranquil waters were, it is not yet possible satisfactorily to detail. The lœss is found in all parts of Ramsey County, but it varies in thickness and in composition. It is thin or wholly wanting in some rolling gravelly tracts, and is very thick in some confined valleys. It is sandy, or graduates downward into sand, in much of the northern part of the county, particularly in Rice Creek valley, and in some places in the bluffs of the Mississippi below St. Paul, and it is fine and somewhat clayey in the high and rolling clay tract in the eastern part of the county, particularly in the eastern part of New Canada. It forms a very fine soil for farm crops. It covers the bowlders and gravelly clay of the real drift. It fills some old valleys—indeed is always thicker in valleys than on the uplands. It is occasionally stratified and passes into sand below in places where agitated water was abundant enough to have moved such materials before the epoch of the loam. In other cases it is placed abruptly immediately over a coarse, gravelly or bowlder-bearing stratum.

In the southwestern corner of the State (Rock and Pipestone Counties) there is a gradual change from stony bowlder-clay to lœss, horizontally, in passing from the Coteau de Prairie (in Lyon and Murray Counties) southward to the Iowa State line. Exposures along the banks of creeks, and the digging of wells, make this plain. There is a gradual loss of bowlders, then of the small stones, then of gravel; and an equally gradual increase of the characteristic features of the lœss,—close, clayey consistency, crumbling in the air like slacking quicklime, and white limy concretions, in some cases the concretions which have been so often mentioned as a peculiarity of

the lœss, are in the same deposit with small gravel stones of northern origin; and pieces of northern limestone. The drift clay, true northern bowlder clay, the product of glaciers, thus changes gradually into a true lœss, the product of aqueous agencies. While this indicates for that locality, at least, a merging of one force into the other, and the slowness of the change, through an interval of about fifty miles in a broad, level, open country, it perhaps gives the key to the events that occurred in other latitudes where the surface was more broken, and where the effects are more complicated by not having all the steps recorded. Just as in the older geological formations, wherever the series is complete, without sudden transitions, the history is best known, so in the history of the drift, where the effects change gradually, are the records of "lost" epochs, and these "beds of transition" need the closest scrutiny, being the only evidence of what transpired between formations which in other regions pass abruptly from one to the other. This here indicates that the age of the lœss was cotemporary with that of the bowlder clay in the Coteau de Prairie. There must be some explanation given for the coëxistence of these forces which spread the loam and those which brought the glacial dirft. In other words, if the loam, which is sometimes a laminated clay, be regarded as the equivalent in age of the fine laminated clays of the great lakes and of other high-water marks in the northwest, which have been referred to a distinct "epoch" by Dana and others (the Champlain), then that epoch was not subsequent in time to the Glacial epoch but cotemporary with it, and its phenomena differ from those of the last Glacial epoch because they have been studied at distant points where they are contrasted, and where the glacial winter operated differently. Where there is an immediate succession of superposition, that fact in the drift does not imply immediate succession in time any more than it does in the Silurian rocks, a fact which has been ignored many times; and hence have resulted a great many special histories and theories. The lœss, for instance, lies on the older drift clay all along the Mississippi valley, and has generally been taken to prove an immediate transition from the drift-epoch to the loam-epoch, when really a long period of time, involving forest growths and the slow on-coming of a Glacial epoch, intervened, the loam itself passing horizontally into the glacial deposits of that epoch.

So in Ramsey County the loam has been seen to follow by insensible gradations from a sand or even a fine gravel, the change here taking place perpendicularly. In this case the coarser deposit below was the result of more copious and more agitated water, as in the bluff-terraces below St. Paul, or in the washed materials in the western part of Reserve township, and the loam the result of the diminution and more quiet state of the same waters. Thus, if the waters which overspread and washed the old drift and formed the gravelly terraces of the Mississippi came from the ice-fields of a contemporary glacier lying further north,

then the waters which spread the loam, a finer deposit, also came from the same source, operating a little later, and with diminished force.

2. *Systematic Geology of the 40th Parallel;* by CLARENCE KING.*—The following are citations from one of the chapters in this valuable volume; it is entitled "Résumé of Stratigraphical Geology," and gives some of the geological conclusions reached by the author.

In the 120,000 feet of sedimentary accumulations the grander divisions of Archæan or Azoic, Palæozoic, Mesozoic, and Cenozoic are distinctly outlined by divisional periods of marked unconformity. Considered as a whole, there is a noteworthy fullness in the geological column. None of the important stratigraphical time-divisions are wanting except those obscure intermediate deposits which in other countries lie between the base of the Cambrian and the summit of the crystalline Archæan series. From the first of Cambrian age to the present every important interval of time is recorded in the abundant gathering of sediments, which are with singular fullness characterized by appropriate and typical life-forms.

As in all other geological fields, the most important interruption of the continuity of deposit was at the close of the Archæan age, and the most striking difference between any two successive groups of rocks is that which characterizes the relations of the Archæan and the Palæozoic. With the exception of a few slates of supposed Huronian age, which the microscope shows to be richly charged with crystallites, all the non-eruptive Archæan rocks have passed from the original condition of detrital beds into sheets or bodies of distinctly crystallized material.

Not only are the Archæan exposures of such frequency over the Fortieth Parallel area as to insure a moderately complete knowledge of stratigraphical sequence and materials of the period, but also, owing to the relations which have been described with the overlying Palæozoic, I am able to reconstruct with considerable accuracy the topographical configuration of the Archæan surface. Supposing all the post-Archæan rocks to be removed, and considering what we now know of the whole area at the close of the Archæan age, the first prominent fact is, that coëxtensive with the greater part of the Cordilleras—that is, from longitude 104° westward as far as the Archæan exposures extend—was a great Archæan mountain system built up of at least two sets of nonconformable strata, referred to Laurentian and Huronian; the lower and older composed of granitoid gneisses chiefly made up of quartz and orthoclase, but carrying a little mica, sparing triclinic feldspars, and chlorite pseudomorphous after garnet and mica.

Over these, whether with actual conformity or not is undetermined, lies an enormous series of mica gneisses rich in quartz and biotite, orthoclase ordinarily exceeding plagioclase. The earlier aplitic gneisses and the later mica gneisses expose about 25,000 feet each of conformable beds.

* See the preceding number of this Journal, page 66.

A third group, nonconformable with the earliest aplitic series, the relations with the intermediate mica-gneiss series being unknown, consists of mica and hornblende schists passing upward into slates, quartzites, limestones, and dolomites. * * *

Upon grounds set forth in Section IV of Chapter II it is clear that the general topography prior to the deposition of the earliest Cambrian rocks was that of a great mountain system, displaying lofty ranges made of crumpled strata, enormous precipices, a result of mechanical dislocation, and, finally, a type of high mountain sculpture of such broad, smooth forms as to warrant the belief that subaërial erosion had never carved and furrowed the mountain flanks with the sharp ravines characteristic of modern mountain topography. East of the Rocky Mountains, in the geological province of the Great Plains, there are no Archæan outcrops; and when we consider the comparative thinness of the later sedimentary beds superposed over that region, the absence of outcropping Archæan masses piercing through the latter sediments is excellent proof that over that area Archæan mountain ranges did not exist. This is important as defining the Archæan Cordilleras within the limits of the modern Cordilleras, or, as is a more strictly correct view, the Archæan Cordilleras have determined not only the general area but much of the local detailed structure of the modern Cordilleras.

The topographical features of the present terrestrial surface are far less grand than the Archæan orography. The great Archæan precipices brought to light in Uinta and Wahsatch ranges are absolutely unparalleled in the topography of to-day. * * *

There is always a complete, sharp, unmistakable nonconformity between the crystalline Archæan topography and the superjacent sediments.

Considered as a whole, the Palæozoic series constituted a conformable body, laid down over the rugged Archæan mountain system. It first appears in the region of the Rocky Mountains with a total thickness of about a thousand feet, the strata surrounding and abutting against permanent Archæan islands, which, during the whole Palæozoic and Mesozoic, were lifted above the level of deposition. Throughout all Palæozoic time only 1,000 feet of strata accumulated over our part of the Rocky Mountains, and we get no glimpses of deeper hollows in which lower Cambrian beds might have been deposited. Passing westward, the series gradually thickens to 32,000 feet in the region of the Wahsatch and about 40,000 feet at the extreme western Palæozoic limit, longitude 117° 30′, where, from the evidences of shore-phenomena, and the non-continuation of the beds westward, we are warranted in assuming the Palæozoic coast. * * *

Viewed regardless of the age of the individual beds, the Palæozoic series can be divided by the character of their materials into four great groups. The first is a purely detrital Cambrian, which, although of comparatively fine sediments, in the presence of occasional conglomerates gives evidence of repeated subsidence.

The second group is the great limestone series, beginning with the Pogonip Cambrian limestone, and extending upward to the top of the Lower Coal Measures for 11,000 feet, only interrupted, in the horizon of the Lower Devonian, by a sheet, from 1,000 to 2,000 feet thick, of fine quartzitic detritus. This enormous group of 11,000 feet of limestone, characterized by abundant pelagic faunæ ranging from the Primordial to the top of the Lower Coal Measures, represents in general an age of deep seas. * * *

At the close of the deep-sea lime-period came a third great stratigraphical division of the Palæozoic — Weber quartzite — a body of pure siliceous detritus from 6,000 to 10,000 feet in thickness, characterized by conglomerates both in the near neighborhood of the granitic islands and close to the Nevada shore.

This is immediately succeeded by the fourth group or Upper Coal Measure limestone, a body about 2,000 feet thick of strictly pelagic material.

The whole Palæozoic, therefore, may be summed up as to its material as two periods of mechanical detritus, interrupted by one and followed by another period of deep-sea lime-formation. While in the conglomerates which appear in all the siliceous members of the series we have evidence of episodes of shallow waters, yet the occurrence of 13,000 feet of limestone indicates enormous intervals of the continued sway of profound ocean. * *

After the close of this great conformable Palæozoic deposition, wide-spread mechanical disturbance occurred, by which the land area west of the Nevada Palæozoic shore became depressed, while all the thickest part of the Palæozoic deposits from the Nevada shore eastward to and including the Wahsatch, rose above the ocean and became a land area. Between the new continent and the old one which went down to the west, there was a complete change of condition. The land became ocean; the ocean became land. In the rising of the Palæozoic, however, the elevation proceeded no farther eastward than the Wahsatch. East of that point, the Upper Carboniferous beds were still the undisturbed ocean-bottom; but instead of receiving sediments either from the destruction of organic life within the ocean area or from the distant continental sources to the west, the newly elevated land-mass, extending from the Wahsatch west to 117° 30′, became the area from which was derived the post-Carboniferous detritus to form the great Mesozoic series that, east of the Wahsatch, were laid down conformably upon the still submerged and still undisturbed Carboniferous.

Upon the western side of the new land-mass, the Archæan continent, having gone down, made a new ocean-bottom, and upon this immediately began to accumulate all the disintegration-products of the new land-mass which the westward draining rivers and the ocean waves were able to deliver. Throughout the Triassic and Jurassic periods the western ocean was accumulating its enormously thick group of conformable sediments upon an Archæan floor, while east of the Wahsatch, in the mediterranean ocean, the

sediments of the Trias and Jura were accumulating conformably upon the Carboniferous; until, at the close of the Jurassic age, there had accumulated in the western sea 20,000 feet, and in the mediterranean sea 3,800 feet, of Triassic and Jurassic material.

The comparison of the Trias-Jura series, in these two separated seas, shows two things: first, that the western sea was very deep during the Trias; secondly, that the mediterranean was shallow during the Trias. * * *

At the close of the Jurassic age, the western ocean, with its original floor of Archæan ranges overlaid by twenty thousand feet or more of conformable Trias-Jura sediments, suffered abrupt orographical uplift, resulting in the formation of a series of sharp folds and elevating a portion of the ocean area, extending from the eastern shore outward and westward as far as the present west base of the Sierra Nevada, making an addition to the continent of 200 miles, the Sierra itself constituting the most western and most elevated of the newly formed mountain ranges. * * *

While this powerful dynamic action was taking place on the west side of the land area, there still remained, so far as upheaval, subsidence, or folding is concerned, a complete calm in the region east of the Wahsatch. The uppermost shaly members of the Jurassic from the Wahsatch out to Kansas are immediately conformably overlaid by the basal members of the Cretaceous. * * *

During Cretaceous time the mediterranean ocean stretched from the eastern base of the Wahsatch into Kansas; and over the entire bottom of that body of water, with the exception of a few Archæan islands, which were still, as they had been throughout the previous ages since the beginning of the Cambrian, lifted above the plane of deposition, a continuous conformable sheet of Cretaceous sediments was laid down. Its greatest thickness was against the western shore of the ocean, namely, against the eastern base of the Wahsatch, where conformably over the top of the Jurassic shales are about 12,000 feet of Cretaceous beds. Passing eastward, this series in the province of the Great Plains near the eastern base of the Rocky Mountain system has thinned to 4,500 or 5,000 feet, and in western Kansas it reaches its thinnest development as described by the Geological Survey of that State. * * *

Throughout the whole Cretaceous, below the top of the Fox Hill, the molluscan fossils are invariably marine, with the exception of certain intercalated groups of purely fresh-water shells near the region of the Wahsatch, which, from their position close to the Cretaceous ocean shore, are evidently the in-washings of a fluviatile fauna.

Regarded as a whole, the basal member is a single sheet of siliceous sediments and rounded conglomerates from 300 to 500 feet thick. Over this lies the great Colorado group, 2,000 feet thick in the west, 1,000 feet thick in the region of the Great Plains, made up chiefly of fine calcareous and argillaceous material, which toward the middle of the group is prominently formed of marles or limestones.

Above the horizon of the Colorado group, the Fox Hill and Laramie are essentially of sandstones, about 9,000 feet in thickness in the region of the Wahsatch, about 3,000 feet in the region of the Great Plains. At the very summit of the uppermost or Laramie group are found Dinosaurs. The fauna up to the base of the Laramie is strictly marine. The Laramie itself carries the remains of an estuarial or brackish-water life, associated with strictly Mesozoic Saurians. With the close of the Cretaceous the conformable series of marine and estuarial deposits east of the Wahsatch come to an end, and the last moments of deposition were immediately followed by one of the most important orographical movements of the whole Cordilleran history.

From the eastern base of the Rocky Mountains to the eastern base of the Wahsatch the whole region was thrown either into wide undulations or sharp folds. So great a range as the Uinta, with its distinct, broad, flat anticlinal, was made at this period. Relatively to the present basin of the Colorado, the whole chain of the Rocky Mountains was elevated so as to define a broad, shallow depression, which now includes the waters of Colorado River. Powerful and important as this orographical movement was, it failed to disturb the coast deposits of the Pacific in California; but from reasons already given it seems probable that the first definition of the Cascade Range was caused by its force. In the general geology of North America the most important result of this immediately post-Cretaceous orographical movement was the elevation of the whole interior of the continent and the complete extinction of the inter-American mediterranean ocean.

From the date of this movement no marine waters have ever invaded the middle Cordilleras, and the subsequent strata are all of lacustrine origin. The effect of this orographical movement was to leave that part of the Cordilleras which falls within our study with a free drainage to the sea, with the single exception of the basin of Colorado River, which, from its configuration, immediately became the receptacle of the vast fresh-water Ute Lake, within whose area accumulated the important Vermilion Creek group, the earliest of the fresh-water Eocene strata. Throughout the entire Eocene period the basin of Colorado River was the theatre of a series of four Eocene lakes, whose deposits — unconformable among themselves, as has already been described — amount in all to 10,000 feet; lacustrine rocks characterized from the bottom to the top by an abundant series of vertebrate life covering the whole lapse of Eocene time. The Eocene of the Fortieth Parallel region was a period of four lakes superposed, the unconformity of their deposits due to four orographical disturbances.

An important orographical movement took place at the close of the Eocene, by which the province of the northern Great Plains and a long, narrow tract of Washington Territory, Oregon, Nevada, and California, lying on the eastern base of the Sierra Nevada and the present Cascade Range, became depressed and received

the drainage of the surrounding countries, forming two extended Miocene lakes. The deposits of the westernmost lake are chiefly the tuffs and rearranged ejecta of volcanic eruption. The deposits of the Plains are the simple detritus from the surrounding lands. The series on the west are over 4,000 feet thick; in the east they are not proved to be over 300 or 400 feet. Both contain abundant and typical Miocene vertebrate life.

The close of the Miocene was signalized by a powerful orographical movement over the area of the western Miocene lake, which threw the beds accumulated on its bottom into folds. Contemporaneously with this movement the Miocene lake of the east, by the subsidence of the surrounding country, increased so as to cover the province of the Great Plains.

The Pliocene opened, therefore, with two enormous lakes, one covering the basin country of Utah, Nevada, Idaho, and eastern Oregon; the other occupying the province of the Plains. The Pliocene deposits of the Plains lake are calcareous and sandy beds, which have no nonconformity in angle of dip with the underlying sheet of Miocene sediment, but which overlap it in every direction. The deposits of the great western lake are nonconformable with the Miocene and immensely overlap it to the east, doubling the area of Miocene sediment. Both of these Pliocene lakes — as do the Miocene — contain the remains of rich faunæ. The eastern lake received a maximum of about 2,000 feet of strata; the western lake has nowhere shown over 1,400 feet.

The close of the Pliocene was signalized by another orographical movement, which threw the sediments of the Great Plains lake into their inclined attitude, dipping 4,000 feet to the east and 7,000 feet to the south from the Fortieth Parallel region. This same orographical movement acted differently upon the sheet of sediments which covered the Pliocene lake of the Great Basin. Instead of tilting the entire lake, it broke in the middle, and the two sides were depressed from 1,000 to 2,000 feet thick, the shores faulting downward. The result of the post-Pliocene movement in the department of the Plains was to give thereafter a free drainage to the sea. The result in the area of the Great Basin was to leave two deep depressions, one at the western base of the Wahsatch, one at the base of the Sierra Nevada, which, in Quaternary times, received the abundant waters of the Glacial period and formed the two lakes that have already been described in the Quaternary chapter.

3. *An Elementary Geology, designed especially for the Interior States;* by E. B. ANDREWS, LL.D., of the Ohio Geological Corps, and late Professor in Marietta College. 283 pp. 8vo. Cincinnati, 1878. (Van Antwerp, Bragg & Co.)—In this little work on Elementary Geology, the author, as his preface remarks, has had especial reference to the Interior portion of the United States, exclusive of the Southern States. Many of the illustrations and facts are accordingly from the formations of the Mississippi Valley. It is still a general review of the science, presenting briefly facts

connected with Lithology and Dynamical Geology, and more fully Historical Geology. It is a popular work on the subject for the general reader and will be found a useful book for the young student. The volume is neatly printed.

4. *An Outline of General Geology, with copious References*, designed for the use of both General and Special students; by THEO. B. COMSTOCK, B.Ag., B.S., in charge of the Department of Geology, Paleontology and Economic Geology in Cornell University. 82 pp. 8vo. Ithaca, N. Y. Printed for the author at the University Press. 1878.—This work is based by the author on the Syllabus of his lectures to the students of Cornell University. It is a brief synopsis of the various branches of general geology, presenting in a condensed way the principles and conclusions with many prominent facts. It is well fitted for use in connection with a course of lectures; and the geological student will also find it valuable as a means of reviewing the subject. The work contains a list of references to various geological publications, including treatises, periodicals, transactions of Societies and memoirs, to aid the student in extending his range of study.

5. *Die Glimmergruppe.* II Theil, von G. TSCHERMAK.—The first part of Professor Tschermak's memoir on the Mica family has already been noticed in this Journal (xv, 150, Feburary, 1878), in that the results of his crystallographical and optical studies were given. To these he has now added a discussion of the chemical composition of the species as previously defined by him. His conclusions are based upon sixteen new analyses made with especial care to avoid the errors contained in many earlier analyses and in consequence of which most of them are rejected by him in his discussions. The following are the principal results adopted by him: (1.) BIOTITES: *Anomite*, composed of $Si_6Al_6K_4H_2O_{24}$ and $Si_6Mg_{12}O_{24}$ in ratios from 1:1 to 2:1; *Meroxene*, composed of $Si_6Al_6K_3H_2O_{24}$ and $Si_6Mg_{12}O_{24}$ in similar ratios. *Lepidomelane:* composition, $Si_6Al_6K_2H_4O_{24}(Si_6Fe_6K_2H_4O_{24})$ and $Si_6Mg_{12}O_{24}$. (2.) PHLOGOPITES: *Phlogopite*, composition, $Si_6Al_6K_6O_{24}$, also $Si_{10}H_8O_{24}$ (with $Si_{10}O_8F_{24}$) and $Si_6Mg_{12}O_{24}$ often in the ratio 3:1:4; *Zinnwaldite* (Cryophyllite), composition $Si_6Al_6K_6O_{24}$ ($Si_6Al_6Li_6O_{24}$ $Si_6Fe_{12}O_{24}$ and $Si_{10}F_{24}O_8$ in the ratio 10:2:3. MUSCOVITES: *Lepidolite*, composition $3Si_6Al_6K_6O_{24}(Si_6Al_6Li_6O_{24})$ and $Si_{10}O_8F_{24}$. *Muscovite* (Damourite), composition $Si_6Al_6K_2H_4O_{24}$ true muscovite, and also this same together with $Si_{10}H_8O_{24}$, in the ratio 3:1 (Phengite); *Paragonite*, composition $Si_6Al_6Na_2H_4O_{24}$. *Margarite*, $Si_4Al_6Ca_2H_4O_{24}$.— *Vienna Academy*, lxxviii, June, 1878.

III. BOTANY.

1. *On Plant-Distribution as a field for Geographical Research;* by W. T. THISELTON-DYER, Assistant Director of the Royal Gardens, Kew, London. 1878, pp. 36, 8vo.—This is a lecture, delivered (we believe) at the Royal Institution, and published originally in the Proceedings of the Royal Geographical Society, Lon-

don. Its teaching is interesting and truly noteworthy. It tells how "Vegetation in any given spot maintains its own only by having solved the problem of existing in the best way under the given circumstances. Introduce a new competitor for a particular site that can solve the problem rather more closely, and the old occupant must needs give way." It intimates that this must have been so all along geological time, and under all changes of climate, land and sea. It pictures the great hosts of plants oscillating between the poles and the equator, their ranks thinning by "the friction attendant on their movement," which has extinguished perhaps whole battalions. It takes a general survey of the prominent characteristics of the great floras, northern, southern and tropical, and of their principal divisions. It brings prominently forward "the opinion that the northern hemisphere has always played the most important part in the evolution and distribution of new vegetable types; in other words, that a greater number of plants have migrated from the north to the south [meaning across the tropics] than in a reverse direction." That proposition (based on the temperate floras) is well sustained by obvious facts, and follows almost of course from the greater amount and longitudinal contiguity of northern lands, as Mr. Darwin has "suspected." But this may probably be limited to the extant vegetation and its nearer predecessors. If the paleontological botanists are at all correct in their ordinal determinations, the reverse might well have been the case at earlier periods, when Proteaceæ and Laurineæ abounded in northern temperate regions. It must needs have been so if there was for any long period a preponderance of southern land with northward extension.

A good part of the lecture—as rich in practical value as the remainder is in theoretical interest—recounts what geographical explorers have recently been doing for botany by collecting materials and information, indicates how very much is yet to be done in this way, how easy it is to collect and preserve botanical specimens, and what important services the "roving Englishman" and still more the desciplined explorer, may render to the botanical studies. A. G.

2. *Conspectus Floræ Europææ*, auctore C. F. Nyman. Orelso Sueciæ, 1878. I. *Ranunculaceæ—Pomaceæ*. pp. 240, 8vo.—This systematic catalogue of European plants, arranged in the Candollean order, with leading references, principal synonyms, and localities, supplies a desideratum, so far as it extends, and the second part, which will include the remaining *Polypetalæ*, is announced as in press. It is evidently a work of critical importance, and is well arranged. A. G.

3. *Botanical Necrology of* 1878.—An unusual number of botanists have deceased in the course of the past year. The first and the last names on the list are venerable.

Elias Magnus Fries, of Upsal, died February 8, 1878, in the eighty-fourth year of his age, a month after the hundredth anniversary of the death of Linnæus at that ancient University. He

was born, as was Linnæus, in the south of Sweden, was educated at Lund, where he was early made demonstrator of Botany, and was translated to Upsal more than forty years ago, where he occupied the chair, not of Botany, but of Practical Economy, answering, we suppose, to Rural Economy. He was, nevertheless, the greatest Swedish botanist since Linnæus, and the last survivor of those whose teachers were taught by Linnæus. He began to publish on Phænogamous Botany in the year 1814, in which department he was high authority to the last; but soon took up Lichens and Fungi, in the latter of which his knowledge was unrivalled and his judgment wonderfully correct, considering that his studies were unaided by the compound microscope. His last work of any consequence was a new edition of his *Hymenomycetes Europæi*, of which he wrote the preface on his 81st birthday, August 15, 1874.

Ludwig Pfeiffer, of Cassel, died at the beginning of the year, at the age of 72. He wrote on *Cacteæ*, and published a Synonymia Botanica.

Andrew Murray, a writer to the signet at Edinburgh, where he was born, died January 10, 1878, at the age of 66. He was an entomologist more than a botanist; but he came up to London to become the acting Secretary of the Horticultural Society, and became learned in *Coniferæ*, publishing a volume on the Pines and Firs of Japan in 1863, and, later, various articles upon the *Coniferæ* of our Pacific Coast.

Andrew Bloxam, an English clergyman, one of the earliest of the critical investigators of *Rubus*,—a genus which perplexes European botanists,—died February 2, 1877, at the age of 76.

François Vincent Raspail, whose name has for many years past been associated with radical politics, was in earlier years a keen but eccentric botanist. His memoir on the Grasses, with a new classification, was published at Paris in the year 1825; his new system of Vegetable Physiology and Botany in 1837. He died at Arcueil, near Paris, January 6, 1877, at the age of 87.

Sulpiz Kurz, Curator of the Calcutta Herbarium, and an active botanist, died at Palo-Penang, January 15, 1878. Age and birth unknown to us; but he was a pupil of von Martius at Munich, and went early to Java, whence he was transferred to British India, and was a valuable explorer. His Forest Flora of British Burma, in two volumes, was published at Calcutta last year, about the time of the author's death.

M. Durieu (de Maisonneuve), long the director of the Botanic Garden at Bordeaux, author of many botanical and vinicultural papers, and of the first (Cryptogamic) part of a Flora of Algeria, died at Bordeaux, Feb. 20, 1878, at the age of 82.

Charles Pickering, M.D., who died in Boston on the 17th of March last, in the 73d year of his age, and to whose memory a tribute has already been paid in this Journal, must be counted among the botanists, although most of his life was devoted to zoology, anthropology, and cognate studies. His extensive

work On the Geographical Distribution of Plants and Animals is a collection of materials for the study of the subject; and a bulky volume entitled Man's Record of his own Existence—the printing of which was in progress when the author died, and which is not yet published—is largely a record of changes in the habitat of plants effected under human agency, from B. C. 4491 down to recent times. It is a monument of wonderful industry.

Moritz Seubert, Professor of Botany at Carlsruhe, author of a Flora of the Azores on Hochstetter's collections and notes, and of a monograph of *Elatineæ*, also of several monographs in the Flora Brasiliensis, died April 6, in the 60th year of his age.

Thomas Thomson, M.D., the well-known associate of Sir Joseph Hooker in Himalayan exploration, and in the Indian Flora, the explorer of Thibet, sometime Director of the Calcutta Botanic Garden,—a botanist whose career of high promise, was sadly cut short by ill health—died in London on the 18th of April last, at the age of 60. He was the son of the distinguished chemist Thomas Thomson, of Glasgow, where the lamented subject of this notice was born and educated.

Giovanni Zanardini, Professor of Botany at Venice, patriarch of Italian Phycologists, died, April 24, at the age of 74.

Roberto de Visiani, Professor of Botany in Padua for many years, author of a Flora Dalmatica, and of many interesting papers, died on the 4th of May, at the age of 77.

Barthelemy Charles DuMortier, of Tournai, Belgium—long eminent as a statesman as well as botanist (the leader of the clerical party in Belgium politics), and greatly esteemed as a man—died July 9, in his 82d year. His earliest papers bear the date of 1822; he published on *Hepaticæ* in 1831, and again in 1871. He published on Grasses in 1823 and again in 1868. He brought out a new and elaborate classification of fruits in 1835.

Elias Borszczow (whose name we could not presume to pronounce), Professor of Botany and Director of the Botanic Garden of Kiew, in Southern Russia,—who in 1858 botanically explored the Aral-Caspian desert, and published a monograph of the plants yielding galbanum and assa-fœtida,—died on the 12th of May.

James McNab, Curator of the Edinburgh Botanical Gardens, as was his father before him, sometime President of the Edinburgh Botanical Society, the most accomplished of cultivators, and a well-informed botanist, died on the 20th of November, in his 69th year. We well remember his visit to the United States in the year 1834.

Stephen T. Olney, of Providence, Rhode Island, died July 27, 1878, at the age of 66. He was for many years one of the most active local botanists of this country; published in 1845 a Catalogue of Rhode Island Plants, with continuations and emendations in 1846 and 1847; a List of Rhode Island Algæ in 1871; in the latter years of botanical activity studied Carices with critical care and minuteness, published several fascicles of Exsiccatæ, admirably prepared; and formed a fine and rich herbarium and

library, which he gave to Brown University. To it also, or at least to his native State, he made handsome legacies for botanical instruction, as well as other benevolent bequests. These benefactions, and his many good offices, should preserve a pleasant memory of a useful life, the end of which was obscured and afflicted by mental trouble. In Botany his name is commemorated by a remarkable Leguminous tree of Arizona (*Olneya*) and by several species of his own discovery. Mr. Olney was unmarried, and was for most of his life engaged in business, at first in Augusta, Georgia, and afterward in his native town.

James Watson Robbins, M.D., died at Uxbridge, Massachusetts (where he resided and was an esteemed physician for the greater part of a long life), on the 9th day of January, 1879, at the age of 77. With the exception of Dr. Bigelow he was the oldest New England botanist, and perhaps the oldest in the United States; and, within his range, he was certainly one of the most careful and accurate. He was a colleague of William Oakes, who had the pleasure of naming several species discovered by him in his honor, and he had an extended correspondence with all our botanists. He collected not only throughout New England, but in Virginia and Maryland, where he resided for several years when a young man, and on the shore of Lake Superior, where he spent four years. Of late, he devoted his attention mainly to aquatic phænogamous plants, especially to the difficult genus *Potamogeton*. He contributed the monograph of this genus to the last edition of Gray's Manual. He first detected that simplest and smallest of flowering plants, *Wolffia*, in this country. His excellence and amiability secured the attachment of all who knew him. He was born at Colebrook, Conn., November 18, 1801, graduated at Yale College in 1822, and there took his medical degree in 1828. In his death we have lost the most critical student of the botany of New England and the Northern Atlantic States.

Jacob Bigelow, the most venerable of botanists, even more distinguished as a physician, a cultivator of the fine and useful arts, and a scholar, one of the most rounded and symmetrically developed men of his time and place, died at Boston, on the 10th of January ult. The notice due to his life and services must be deferred to the next number. A. G.

IV. Miscellaneous Scientific Intelligence.

1. *Technologisches Wörterbuch;* I. Deutsch-Englisch-Französisch; bearbeitet von E. Althans, L. Bach, u. A., herausgegeben von Carl von Albert, mit einem Vorwort von Dr. Karl Karmarsch. Dritte verbesserte und bedeutend vermehrte Auflage. 743 pp. 8vo. Wiesbaden, 1877. (J. F. Bergmann; B. Westermann & Co. in New York.)—This Technological Dictionary deserves high commendation both for its completeness and its accuracy. The subjects which it embraces include all the prominent branches as well of pure as of applied science, so that the work is alike valu-

able to the student and the manufacturer, the technologist and the merchant. This volume gives the English and French equivalents of the German technical and scientific words and expressions; the other two volumes of the series are arranged for English and French readers respectively. The names of seventeen specialists are given, who have assisted in the preparation of this the third edition of the first volume, and they alone are a sufficient proof of the excellence of the work.

2. *Chromometry.*—Under this name Professor Kœnig describes a new branch of quantitative analysis with the blowpipe. The method depends upon the observations of the color produced in the borax bead by various metallic oxides, with the "Chromometer"; in the hands of the describer it is made to yield very accurate results.—*Amer. Soc. Philadelphia*, Oct. 4, 1878.

3. *Additional characters of the Sauropoda;* by O. C. Marsh.—Beside the characters of the *Sauropoda*, already made known by the writer,* others of importance have been since brought to light.

The skull, of which so little has been known hitherto in the Dinosaurs, presents in *Morosaurus grandis* some strongly lacertian characters. It is short, high and narrow, something like that of the Chameleon. The supra-occipital is very large, and forms the upper part of the foramen magnum. The ex-occipitals have long par-occipital processes. The occipital condyle is formed entirely of the basi-occipital. The long basipterygoid processes are of the lacertian type. The quadrate is elongated, very slender above, and has a small articular head. Below, it is fixed by the pterygoid, which unites with it by suture. On the outer side of the quadrate, below the middle, the quadrato-jugal joins it by suture. Its posterior end is cup-shaped, with the cavity opening outward, and partially closed by a thumb-like process. This bone divides in front, one branch going upward, and the other forward, to join the jugal and complete the lower arch. The bones of the cranium were united by open sutures, and there are large parietal fossæ. The orbits are large, and there is a vacuity in front of the lachrymal. The upper jaws are especially short and deep, and each maxillary contained nine teeth. The nasals and premaxillaries were narrow. The lower jaws were not coössified at the symphysis, and each dentary bore twelve teeth. This bone was very deep in front, and pointed behind.

Episternal bone.—A bone, found with the remains of *Apatosaurus ajax*, so strongly resembles the episternal element in lizards, that it must be regarded as an episternal bone. It is cruciform in shape, and symmetrically bilateral. The posterior process is abruptly truncated; the anterior is short and obtuse, and the lateral processes are the longest. The lower surface of this bone is slightly convex on the median line, and more strongly convex transversely. The upper surface and sides indicate that it was surrounded mainly by cartilage. By this interpretation of the bone, the posterior truncated process abutted against the sternum,

* This Journal, vol. xvi, p. 411, and vol. xvii. p. 86.

the posterior reëntrant angles were met by the coracoids, or the intervening cartilage. This would leave the anterior concavities for clavicles, no evidence of which has hitherto been found in Dinosaurs. Their existence in *Apatosaurus*, if not in other *Sauropoda*, seems therefore a necessity. The large rugose facet on the anterior projection of the scapula offers the natural place for union with that bone. No clavicles were found with this episternal bone, but a single specimen from a neighboring locality agrees closely with what we should expect the corresponding clavicle to be. These interesting remains will be more fully described by the writer in another communication.

Yale College, January 22, 1879.

4. *Portrait of Humboldt.*—An excellent portrait, of natural size, painted by Steuben, belongs to Madame de Schœnfeld, of Paris, and is to be disposed of. It should be in some of the Galleries or Museums of this country. Information respecting it may be sought of the distinguished botanist, E. Cosson, 7 Rue Abbatucci, Paris.